MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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W. H. MAW, Esq., PRESIDENT, in the Chair.

Major Alexander Davidson Fleming, Artillery Mansions, 75 Victoria Street, London, S.W.,

was balloted for and duly elected a Fellow of the Society.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended :—

Edgar T. Adams, 5 Warkworth Street, Cambridge (proposed by E. T. Whittaker);

Robert Jonckheere, Observatoire Stella, Roubaix, France (proposed by Camille Flammarion);

John Stewart, Chief Officer, R.M.S. "Empress of China," The Willows, Wallasey, Birkenhead (proposed by E. B. Knobel); and

Samuel Veevers, Normanton, Kimberley Drive, Great Crosby, near Liverpool (proposed by R. C. Johnson).

Seventy-seven presents were announced as having been received since the last meeting, including, amongst others :—

W. Bramsen, Japanese Chronological Tables, presented by E. B. Knobel; Optical Convention, Proceedings of the Meeting May–June, 1905, presented by the Committee; Oxford Astrographic Catalogue, vol. i., presented by the University Observatory, Oxford; J. A. Parkhurst, Researches in Stellar Photometry, presented by the author; Royal Observatory, Greenwich, Astronomical Observations, etc., presented by the Observatory; Royal Observatory, Cape of Good Hope, Annals, vols. x., xii., Astrographic Catalogue, etc., presented by the Observatory.

Astrographic Chart: 20 charts, presented by the Royal Observatory, Greenwich, and 19 charts presented by the San Fernando Observatory. Photographs of the spectrum of Mira Ceti, presented by the Rev. W. Sidgreaves.

On the Possibility of Improving the Places of the Reference Stars for the Astrographic Catalogue from the Photographic Measures. By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

1. On each of the plates taken for the Astrographic Catalogue there are certain stars of which meridian observations have been made, and the constants of the plate are found by using these recorded places. But the places are often defective, from errors of observation and accumulated proper motions; and the errors are indicated by the residuals found on comparing the photographic measures (reduced with the plate-constants found by using all the stars) with the individual meridian places. But these residuals cannot be taken as satisfactory corrections to the adopted places, because the plate-constants, having been found from faulty meridian places, are themselves faulty.

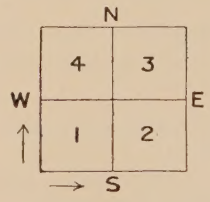


FIG. 1.

2. Taking only a single plate, if we correct the adopted places by the residuals found, and then solve for plate-constants again, we shall get precisely the same constants as before, and the residuals now will be all zero; but the improvement is of course only fictitious, and no real advance has been made. We need not, however, restrict ourselves to a single plate. Every star occurs on at least two plates, and we get at least two different residuals for it. Moreover, the stars in the four quarters of any plate will, as a rule, be on four different overlapping plates. Shall we then adopt as corrections to the original places the means of the residuals for each star (usually two, but sometimes more), and then determine the plate-constants afresh? There is no *prima facie* objection to this course; but it is questionable whether it is the best possible, for the following reason.

3. Call the residuals determined from the plate itself A, and those from other plates B: it is proposed to use $\frac{1}{2}(A+B)$. Now the portion $\frac{1}{2}A$ is simply non-effective. It has been already remarked

that if the residuals A be applied to the original places, and the constants redetermined, they will be the same as before; in other words, if we determine the *corrections* to a, b, c by solving a series of equations such as

$$\Delta a.x + \Delta b.y + \Delta c = \mathrm{A}$$

we shall get $\Delta a = 0$, $\Delta b = 0$, $\Delta c = 0$. Naturally we shall get the same zero result if we write $\frac{1}{2}$A instead of A on the right of all the equations. Hence it may be argued that we may as well save ourselves the trouble of introducing the system of residuals A into the equations at all, since they do neither good nor harm; with one possible qualification. Some stars occur on more than two plates, and if we form the mean of all plates, the residuals A will have a factor $\frac{1}{3}$ or $\frac{1}{4}$ in some cases instead of $\frac{1}{2}$. But there is clearly no systematic advantage to be gained by such exceptions, and we need not discuss them in detail.

4. The question therefore arises whether it is not better to exclude the residuals A altogether, since they do neither good nor harm in improving the plate-constants. Should not the corrections to the originally adopted places be determined from residuals B entirely?—*i.e.* instead of neglecting the $\frac{1}{2}$A as useless, and retaining the $\frac{1}{2}$B, should we not substitute B simply? or the mean of several B residuals if they are available? The answer to this question can only be arrived at by considering in some detail what the residuals represent.

5. For each star we write down two equations of the form

$$ax + by + c - \mathrm{X} = 0, \quad dx + ey + f - \mathrm{Y} = 0$$

and it will be sufficient to consider the first of these equations only, since the procedure is the same for both. In this equation x and y are the co-ordinates of the star, a, b, c the plate-constants to be determined, X the difference between the measured and calculated standard co-ordinates of the star. There will be as many equations as there are known stars on the plate, and they may be solved by least squares or any equivalent process. At Greenwich and Oxford the labour of least squares has been avoided by taking the mean of all the equations for the S and N halves of the plate and for the E and W halves; *i.e.* the four equations

$$\begin{aligned}
\{[1]+[2]\}/(n_1+n_2) &= 0\\
\{[4]+[3]\}/(n_4+n_3) &= 0\\
\{[2]+[3]\}/(n_2+n_3) &= 0\\
\{[1]+[4]\}/(n_1+n_4) &= 0
\end{aligned}$$

have been formed; where [1] represents the sum of all the expressions

$$ax + by + c - \mathrm{X}$$

in the SW corner of the plate marked 1 in fig. 1: and n_1 is the number of stars in this portion. Next, the constant c is eliminated

by subtracting the second equation from the first and the fourth from the third, and we get the pair

$$\{[1]+[2]\}/(n_1+n_2) - \{[4]+[3]\}/(n_4+n_3) = 0$$
$$\{[2]+[3]\}/(n_2+n_3) - \{[1]+[4]\}/(n_1+n_4) = 0$$

from which to determine a and b.

6. If the stars are equally distributed in the four quadrants, so that $n_1 = n_2 = n_3 = n_4$, these equations reduce to

$$[1]=[3] \quad \text{and} \quad [2]=[4]$$

The discussion thus divides itself into two parts: first, the consideration of what happens when the stars are uniformly distributed; and secondly, the effect of irregular distribution.

7. We shall take uniform distribution first; and we may take the very simplest case of it, viz. when there are just four stars placed at the centres of the four quadrants. If the side of the plate be $4s$, the co-ordinates of these stars referred to the centre of the plate as origin will be (see fig. 1)

1	$x=-s$	$y=-s$
2	$x=+s$	$y=-s$
3	$x=+s$	$y=+s$
4	$x=-s$	$y=+s$

and the equations for finding a and b become

$$-2s.a - 2s.b = X_1 - X_3$$
$$+2s.a - 2s.b = X_2 - X_4$$

Thus

$$4s.a = -X_1 + X_2 + X_3 - X_4 \quad . \quad . \quad . \quad (1)$$
$$4s.b = -X_1 - X_2 + X_3 + X_4 \quad . \quad . \quad . \quad (2)$$

and for c we add all four equations, so that

$$4c = X_1 + X_2 + X_3 + X_4 \quad . \quad . \quad . \quad . \quad (3)$$

8. The residual X_1 is reduced, with these values of a, b, and c, to

$$X_1 - (ax + by + c)$$

which on putting $x=-s$, $y=-s$, and the values of a, b, c given above becomes

$$\tfrac{1}{4}(X_1 - X_2 + X_3 - X_4) = +I, \text{ say} \quad . \quad . \quad . \quad (4)$$

9. The residual for the third quadrant is also reduced to $+I$

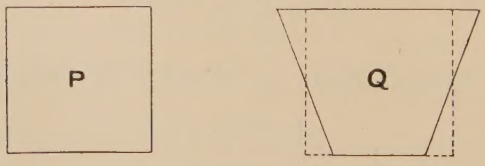


FIG. 2.

while the residuals for the second and fourth quadrants become $-I$. This quantity I, therefore, cannot be removed by a linear solution.

Its effect is to alter the square P (fig. 2) into the trapezium Q; and it may be produced, not by erroneous adopted places, but by a "tilt" of the plate; or, in other words, by our assuming the wrong plate-centre to which to refer our standard co-ordinates. It is known that this modifies ξ, η into expressions of the form

$$\xi' = \frac{(1+a)\xi + b\eta + c}{1 - k\xi - l\eta}$$

$$= (1+a')\xi + b'\eta + c' + k\xi^2 + l\xi\eta \text{ approx.}$$
$$\eta' = d'\xi + (1+e')\eta + f' + k\xi\eta + l\eta^2 \quad ,,$$

where (k, l) are the co-ordinates of the true centre. The terms which give rise to an I effect are the $\xi\eta$ terms, which are positive in the first and third quadrants and negative in the second and fourth. But it is known from experience of plates, in parts of the sky where stars are numerous, that no very large part of I is due to tilt in this way.

10. It is perhaps also as well to verify that the same I term would be given by the method of least squares, at any rate in the simple case at present under consideration. Solving the equations

$$\begin{aligned} -as - bs + c &= X_1 \\ +as - bs + c &= X_2 \\ +as + bs + c &= X_3 \\ -as + bs + c &= X_4 \end{aligned}$$

by least squares, the normal equations are

$$\begin{aligned} 4s^2a &= s(-X_1 + X_2 + X_3 - X_4) \\ 4s^2b &= s(-X_1 - X_2 + X_3 + X_4) \\ 4c &= (\ X_1 + X_2 + X_3 + X_4) \end{aligned}$$

which are precisely the same as those obtained in § 7. Hence the constants and residuals obtained are the same, and we still get the residuals $+I$, $-I$, $+I$, $-I$ for the four quadrants where

$$4I = X_1 - X_2 + X_3 - X_4$$

11. It is convenient to have a name for the quantity I, and the name "inconsistency" will be adopted in the present paper for convenience. When I is not zero, *no* linear solution will fit the plate. We can find the value of I before applying any linear solution at all, since it is not altered by the application of linear terms.

12. Now it is clear that the procedure indicated in §§ 2 and 3, of taking the means between residuals from different plates, will not in general reduce the "inconsistency" for any plate to zero. It may reduce it, but even this is not certain. Calling the inconsistency of any plate I_0, and of the four overlapping plates

$+I_1$, $-I_2$, $+I_3$, $-I_4$, then the process of taking the mean of residuals substitutes for I_0 the quantity

$$\tfrac{1}{4}\{\tfrac{1}{2}(I_0+I_1)+\tfrac{1}{2}(I_0+I_2)+\tfrac{1}{2}(I_0+I_3)+\tfrac{1}{2}(I_0+I_4)\}$$
$$=\tfrac{1}{2}I_0+\tfrac{1}{8}(I_1+I_2+I_3+I_4)$$

The first term represents a certain gain, since I_0 is halved; but the second term may either increase or diminish the first numerically. On the average we shall perhaps not advance beyond halving the original inconsistencies. We can, of course, proceed to a second approximation and halve them again; and to a third. And by using the invariant property of the inconsistency we can make these approximations without performing the solutions on the plates if we simply have the inconsistencies for all the plates tabulated before us. It is even easy to summarise this process of approximation algebraically and combine several steps in one. But probably it will be better in practice to watch the process numerically. The end to be attained is the reduction of the inconsistencies to small quantities, zero for choice. And the question is suggested, Is there any simple process for making them all zero *en bloc*? If we knew the correct places of the stars and had perfect photographic measures, the inconsistencies would be all zero. The measures are not perfect, but the errors of measurement may be assumed small compared with the quantities given below; and the problem is to find a set of places for the standard stars to fit them as closely as possible.

13. We may still suppose the standard stars to be arranged in exact rows and columns, four on each plate, and each one common to two plates. Let us represent a set of advantageous corrections to the adopted places of these stars by the subjoined scheme:—

$$\begin{array}{cccc} a_1 & a_2 & a_3 & a_4 \quad . \quad . \quad . \quad . \\ b_1 & b_2 & b_3 & b_4 \quad . \quad . \quad . \quad . \\ c_1 & c_2 & c_3 & c_4 \quad . \quad . \quad . \quad . \\ d_1 & d_2 & d_3 & d_4 \quad . \quad . \quad . \quad . \\ . & . & . & . \quad . \quad . \quad . \quad . \end{array}$$

so that the four stars on one particular plate, for instance, require the corrections b_2, b_3, c_2, c_3. Then if we reduce the inconsistency of every plate to zero we have a series of equations of the form

$$-b_2-c_3+b_3+c_2=4[B_2C_3] \quad . \quad . \quad . \quad (5)$$

where the symbol $[B_2C_3]$ is used to represent the inconsistency.

14. Let n be the number of plates, and therefore of such equations. There are four images on each plate, and therefore altogether $4n$ star images; but since each star occurs on two plates, there will be only $2n$ stars altogether. Hence we have n equations among $2n$ quantities, and there are any number of ways of satisfying them. If we make a single assumption for each plate we get n new equations and all the corrections become determinate.

15. Suppose we make the assumption

$$b_2 + c_3 + b_3 + c_2 = 0 \quad . \quad . \quad . \quad . \quad (6)$$

that is, that the mean correction to the places on any plate is zero, or the plate-constant is undisturbed. This seems a natural assumption to try, at any rate in the first instance. Then, combining the two equations (5) and (6) for the same plate, we get

$$b_2 + c_3 = -2[B_2C_3] \quad (b_3 + c_2) = 2[B_2C_3] \quad . \quad . \quad (7)$$

that is, the inconsistency is divided equally between pairs of opposite quadrants.

16. Now if we follow the diagonal line of stars $a_1\ b_2\ c_3\ d_4$ we have a series of equations giving $a_1 + b_2$, $b_2 + c_3$, $c_3 + d_4$, and so on. Hence if we know any one correction a_1, we find successively $b_2\ c_3\ d_4$ and all the rest.

17. It is to be noted that the case is not the same for the adjacent diagonals in the same sense $b_1\ c_2\ d_3 \ldots$ and $a_2\ b_3\ c_4 \ldots$. There are, according to the scheme adopted for the Astrographic Catalogue, no plates running with corners in this direction—no plate $c_2\ c_3\ d_3\ d_2$ for instance. In covering the sky twice over, we select two only out of four possible ways of covering it; for considering any single plate $b_2\ c_2\ c_3\ b_3$, let us characterise it by the star at the left-hand top corner, in this case b_2. We can cover the sky completely by plates $b_2, b_4, b_6, \ldots . d_2, d_4, d_6 \ldots .$ and so on, without using systems in which $b_3\ c_2\ c_3$ are represented at all. And by the scheme adopted for the Astrographic Catalogue, the overlapping plates would be the system c_3, including plates $c_1\ c_3\ c_5 \ldots ., e_1, e_3, e_5 \ldots .$ but not including c_2 or b_3. The systems b_3 and c_2 would overlap the others in a different way. Had these additional plates been taken, we should have had $2\ n$ equations for inconsistency of type (5) instead of n only; and we could have determined the corrections to the star places completely from these alone, without any additional assumptions.

18. The absence of the plates of type b_1 and c_2 prevents the formation of equations for $b_1 + c_2$, $c_2 + d_3$, etc., but we get b_1 and c_2 from the diagonals of the other sense, $a_2\ b_1$ and $a_4\ b_3\ c_2\ d_1$, and so on. Hence if we know or assume all the a's, we can find all the other terms. Or similarly if we know or assume $a_1\ b_1\ c_1\ d_1 \ldots .$ we could follow the diagonals from these as starting-points and find all the quantities.

19. But it will throw light on the validity of the assumption of equations (6) if we take an actual example. We have hitherto been assuming an ideal distribution of stars, one and one only at the centre of each quadrant. In passing to an actual example, we must replace each ideal star by a number of actual stars scattered over the quadrant, and for a single residual we substitute the mean of several.

20. But these changes will be understood without further explanation, and the following example closely represents an actual

instance which occurred in the usual course of reduction of plates taken at Oxford in zone + 29°, at R.A. $4^h\ 7^m$, where the stars are few in number. The original solution for plate-constants gave residuals for some stars differing by quantities such as $0\cdot006 = 1''\cdot8$ and $0\cdot009 = 2''\cdot7$ from the residuals found from adjacent plates. A new solution for plate-constants was accordingly made by least squares without removing these anomalies; and various assumptions as to error in scale value, etc. were tried without success.

21. In the light of the considerations above advanced, the inconsistencies of this plate and of overlapping plates were calculated with the following approximate results (the precise definition of

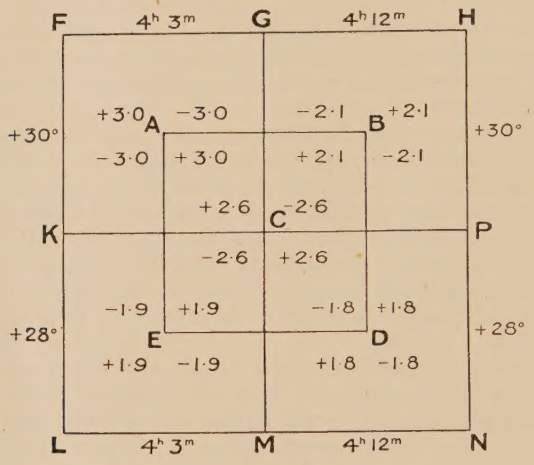


FIG. 3.

inconsistency for stars casually distributed has not yet been settled, and hence the results are provisional only):—

Zone + 30°, For R.A. $4^h\ 3^m$ I = − 3·0, For R.A. $4^h\ 12^m$ I = + 2·1
Zone + 29°, For R.A. $4^h\ 7^m$, I = − 2·6
Zone + 28°, For R.A. $4^h\ 3^m$ I = + 1·9, For R.A. $4^h\ 12^m$ I = + 1·8

The unit for I is $0''\cdot3$ or ·001 of a réseau interval. It will be seen that the plates overlap by 4^{min} in one direction and 5^{min} in the other, and are thus not strictly divided into quarters; but let us neglect this for the present. The values for I indicate that the mean residuals (observed—adopted) in the quadrants are disposed as in fig. 3.

22. In the quadrant AC the agreement of the two plates is good, but in none of the other quadrants is it at all satisfactory. No linear solution can improve it, for such solutions do not alter I; and it will at once be understood why solving by least squares and altering the scale value by some assumption are equally unavailing.

23. We must begin by *assuming* some corrections to one set of outside quadrants. In default of other knowledge, and as an

illustration merely, let us assume *zero* corrections for the top row of quadrants FA, AG, GB, and BH.

Then the correction to AC must be +6·0, in order to take up the +3·0 and +3·0 in the two quadrants FA, AC.

The correction to CD will be

$$+2\cdot6+2\cdot6-6\cdot0=-0\cdot8$$

and to DN
$$-1\cdot8-1\cdot8+0\cdot8=-2\cdot8$$

Similarly following the diagonal HL we get corrections to

$$\begin{aligned} HB &= 0 \\ BC &= +2\cdot1+2\cdot1+0=+4\cdot2 \\ CE &= -2\cdot6-2\cdot6-4\cdot2=-9\cdot4 \\ EL &= +1\cdot9+1\cdot9+9\cdot4=+13\cdot2 \end{aligned}$$

The increase in this last set is alarming, and suggests a revision of the original assumptions; but we will first finish the example. The central plate is now to be revised as follows:—

We correct the adopted places of the stars in the four quadrants by the following quantities

$$\begin{matrix} +6\cdot0 & +4\cdot2 \\ -9\cdot4 & -0\cdot8 \end{matrix}$$

and then solve the equations for scale values and orientation again. The effects on the constants a and b are shown in equations (1) and (2) of § 7; that on c was arranged to be zero. But what immediately concerns us is that when this new linear solution is applied, the mean residuals in the four quadrants will now be zero; for we saw in § 7 that the effect of the corrections above tabulated, taken by themselves, is to leave us with four numerically equal residuals of the value

$$\pm\tfrac{1}{4}(-9\cdot4+4\cdot2-6\cdot0+0\cdot8)=\pm2\cdot6$$

and these will just neutralise the residuals shown by the original solution. Indeed, it is the basis of the method to reduce these residuals to zero. The same will be true of other plates, and hence the discrepancies between adjacent plates will disappear.

24. This will only be true in the mean, *i.e.* at the centre of each quadrant. For stars away from these points errors in a and b will still introduce discrepancies, but they will be naturally smaller. One thing at least is clear: in default of some system of correction of the kind above indicated, there are bound to be discrepancies between adjacent plates in thinly starred regions, due to mistakes neither of measurement nor reduction, which cannot be removed by any change of the linear solution based on the original system of star places. To get improvement we must alter these places relatively to one another; and at first sight this seems alarming. But, after all, is it more drastic than any other modification suggested by the residuals?—taking the mean of the residuals for various plates, for instance?

25. As regards this latter process, we may note in passing how ineffective it may be in helping us to remove discrepancies between plates. Recurring to fig. 3 (p. 114) we see that the corrections suggested by taking the means from adjacent plates are as below, those suggested by observing the inconsistency being put alongside for comparison:

By "means"		By "inconsistency"	
+2·8	−0·3	+6·0	+4·2
−0·3	+0·4	−9·4	−0·8

The set obtained by "means" would leave us, after a new solution, with residuals of numerical value

$$\pm\tfrac{1}{4}(-0\cdot3-0\cdot3-2\cdot8-0\cdot4)=\pm0\cdot9$$

for this plate, which is certainly less than the $\pm2\cdot6$ we started with; but to get rid of the discrepancies altogether we apparently require quite a different set of corrections.

26. We return now to the assumptions made in deriving these corrections, which can obviously be improved. Let us first consider the effect of a different set of starting-points in the top row of quadrants. The diagonal H B C E L, for instance, gives us the series

$$0,\ +4\cdot2,\ -9\cdot4,\ +13\cdot2$$

which is becoming alarmingly large. How much of the increase is due to the assumption of zero as a starting-point? It is easily seen that if we start with $+a$, the effects on the series will be

$$+a,\ -a,\ +a,\ -a$$

and thus if we put $a=+6\cdot6$ we get the series

$$+6\cdot6,\ -2\cdot4,\ -2\cdot8,\ +6\cdot6$$

Thus, if the sum of one set of alternate terms in a diagonal differs from that of the other set, we can reduce the difference to zero.

27. Consider next the other set of assumptions represented by the equation (6) of § 15,

$$b_2+c_3+b_3+c_2=0$$

If we make the alternative assumption

$$b_2+c_3+b_3+c_2=4k\ .\quad .\quad .\quad .\quad (8)$$

then combining with the equation for inconsistency

$$-b_2-c_3+b_3+c_2=4[B_2C_3]$$

we get

$$b_3+c_2=2\{[B_2C_3]+k\}\ .\quad .\quad .\quad .\quad (9)$$
$$b_2+c_3=2\{-[B_2C_3]+k\}\quad .\quad .\quad .\quad (10)$$

If we have started by assuming $a_1\ a_2\ a_3\ a_4$ etc., then we deduce the b's; and equations (9) and (10) serve to deduce the c's. From this

point all the other corrections in two diagonals will be modified by the quantity k, with alternately positive and negative signs. Thus if the two diagonals originally read

	0·0		0·0
	+6·0	+4·2	
	−9·4	−0·8	
+13·2			−2·8

we can add the same quantity, say +5·1, to both −9·4 and −0·8 provided we subtract it from +13·2 and −2·8, and add it to the terms next to these, and so on. The few terms given above will then read

	0·0		0·0
	+6·0	+4·2	
	−4·3	+4·3	
+8·1			−7·9

28. It is clear that we have ample facilities for devising sets of corrections to the original places which shall make overlapping plates accordant; the question now seems to be how to limit our choice. It seems probable that in practice the limitations will arise from the necessity of satisfying known conditions for particular stars. Running down a diagonal in the manner indicated, we shall come to stars with well-determined places and known proper motions, and the proper correction to the adopted place will be known within narrow limits. Such a method cannot be discussed in general terms, but will be special in each particular case; though experience of several cases may suggest some general remarks. But it seems clear that it will serve a useful purpose to calculate the "inconsistency" of every plate, to exhibit the results in diagrammatic form, and then formulate an empirical set of corrections. This work has been put in hand at Oxford, but will naturally take a little time to complete.

29. Meanwhile, it seems desirable to publish this note for the following reason. We have formed at Oxford ledgers of the corrections to the standard stars from all the plates on which they occur, and have done much work in examining the larger discrepancies. In many cases, of course, they have been traced to mere numerical errors (comparatively seldom to errors in measurement), but a sensible number have persistently defied explanation, and yet are so large as to suggest an error. It is now realised that these are due to "inconsistency" of the plate as above, and that the time spent in looking for errors or for better plate-constants was so much time lost. Before continuing the examination of large discrepancies, we propose now to tabulate the inconsistency for each plate and see how many discordances can be removed by suitable assumptions. It seems possible that a note of warning may save the time also of others who may have encountered similar puzzling discrepancies; and this is perhaps a fair excuse for publishing an incomplete investigation.

30. [Paragraphs 30 to end added December 3.] The above paragraphs (except 11 and 12 which have been expanded) were circulated in proof to several astronomers, and various criticisms received. From some of these I recognise that undue prominence has been given to the methods indicated in §§ 13 to 27. It was not intended to represent them as satisfactory (this should be clear from §§ 28 and 29); but in that case they might have been curtailed.

31. Further, it is well remarked by Professor Dyson that there may be independent methods of improving the plate-constants, *e.g.* we may be able to assume that a and e, the constants for scale value, are known from other plates; while b and d, the orientation constants, strengthen one another.

32. But the main point suggested for consideration is untouched. To make the point clear, I venture to restate it as follows, modifying the statement so as to take account of § 31.

(*a*) Find by any method the best linear solution for a plate. Denote the mean residuals in the four quarters by $x_1\ x_2\ x_3\ x_4$; and let

$$4\mathrm{I} = x_1 - x_2 + x_3 - x_4$$

Then I will be the same whatever linear solution is applied, or before any is applied at all; and if I is not zero, *no* linear solution will fit the *adopted* places.

(*b*) If we regard $x_1\ x_2\ x_3\ x_4$ as the proper corrections to the adopted places of the stars, then after applying them the new value of I will be zero; and we have a set of adopted places which can be fitted by a linear solution, viz. the solution already found.

(*c*) But we must take account of overlapping plates, and these do not indicate, in general, the same corrections: they give, say, x'_1, x'_2, x'_3, x'_4. Shall we then adopt as corrections

$$\tfrac{1}{2}(x_1 + x'_1),\ \tfrac{1}{2}(x_2 + x'_2),\ \tfrac{1}{2}(x_3 + x'_3),\ \tfrac{1}{2}(x_4 + x'_4)\ ?$$

This procedure has the obvious advantage that we have only one correction for each star instead of two discordant ones; but it has the serious disadvantage that the inconsistency for each plate is not made zero.

Hence the corrections cannot be regarded as final. When we apply them to the adopted places and make new solutions for each plate we shall have the same situation as at first: the residuals from overlapping plates for the same star will not agree.

(*d*) We can, of course, repeat the process: again take means of the residuals, and again find new constants. We can make a third approximation and a fourth; and we have no guarantee that the process is not endless. A *necessary condition* of our having at last found satisfactory places is that the value of I for each plate should be zero, or at least so small that it can be explained in other ways—tilt, defective measures, etc. But, as indicated in § 12, we

have no guarantee that the inconsistencies will be reduced by this method: they may even be increased.

(*e*) We can only find by actual trial whether this is or is not the case. When we have tabulated the inconsistencies, we can very quickly test the effect of taking means of overlapping plates without performing any actual solutions. If the process is convergent, we can adopt the results to which it leads just as satisfactorily as we can take means between two plates in the first instance: if the process is not convergent, the first step is as wrong as any other; and we detect places where new meridian observations are *imperative* if any advance is to be made. Needless to remark, additional meridian observations will be always welcome and helpful; but they may not be immediately forthcoming.

Pogson's Observations of U Geminorum. Edited by H. H. Turner, D.Sc., F.R.S., Savilian Professor.

1. In 1904 April Mr Joseph Baxendell, of Southport, put into the writer's hands a number of MSS., notebooks and charts, representing the work of his uncle, Mr N. R. Pogson, on Variable Stars. Mr Baxendell found that he had not the leisure necessary to arrange the material for publication, and expressed the hope that the editing of Knott's and Peek's observations (*Memoirs R.A.S.*, vols. lii. and lv.) might be extended to Pogson's. A small grant from the Government Grant Fund enabled me to get the original notebooks copied out, as a safeguard against loss or injury, and the charts were photographed by Mr Bellamy and the originals deposited with the Royal Astronomical Society. But as yet I have made but little headway with the actual copy for press. The material requires more study than might be expected, some of it being very scrappy.

2. A recent announcement of a prize question on the variable U Geminorum, by the University of Utrecht, produced some inquiries for Pogson's original observations of this curious variable, and turned my thoughts in a new direction. Instead of trying to deal with the whole material at once, which would require at least several weeks' continuous attention, could the observations of individual stars be published separately? As an experiment, the observations of U Geminorum have been collected as below, and their immediate publication will in any case serve the good purpose of putting this valuable early material in the hands of those now undertaking a discussion of this remarkable star.

3. Pogson began to look for this star soon after Hind's discovery on 1855 December 15, and his first observation of it was dated 1856 March 26, when he makes this note:—

> The variable subject to strange fluctuations at intervals of 6 to 15 seconds, and quite to the extent of 4 mags. The neighbouring small stars were steady, not at all twitching like

the variable. The phenomenon (which was quite new to me) was watched for above half-an-hour with powers 54, 65, and 95. At times it quite vanished, and then surpassed the comparison star *a*.

On the next night, 1855 March 27, he notes:

Far from steady, but the pulsations much less marked than last night.

And he seems to have looked in vain for a repetition of the fluctuations of his first observation. Thus he notes on—

1857 April 15. A fine sky, but definition rather unsteady. U is certainly less steady than the neighbouring stars of less magnitude, but not in the marked manner previously observed.

1859 February 16. No extraordinary appearance different to neighbouring stars, except that it was rather less sharply defined than they were; the colour was a leaden white, not at all red.

With these exceptions the notes made are not of great interest. Many of them are simply "fine sky," or "moonlight," or "passing clouds"; and it seems unnecessary to encumber the record with them. Those likely to prove of value are here collected.

1857 April 7. The south 93 certainly brighter than the north.

1857 April 18. In a splendid sky, just suspected.

1857 October 30. Well compared: certainly on the rapid increase.

1864 September 28. Star 124 very little less than 113.

1866 April 16. Well seen and compared at 8 p.m. About 10½ p.m. decidedly brighter.

1868 November 17, 18. Either U or star 142 of Baxendell seen.

1870 January 17. Bright yellow; moon totally eclipsed.

4. The telescopes used by Mr Pogson at various times and places were as follows:—

At the Radcliffe Observatory, Oxford, to end of 1858.

Designation.	Aperture in inches.	Focus in inches.	
E	7·2	120	Equatorial at Radcliffe Observatory.
R	2·2	30	Ramsden portable telescope.
Dd	3·8	42	Dollond portable telescope.
SL	3·8	60	"Smythian" or "Lee": acquired from Adm. Smyth by Dr Lee of Hartwell, and lent by him to Mr Pogson in 1857 (Speculum Hartwellianum). Taken back to Hartwell in 1859 by Mr Pogson and used in his house. Apparently taken also to Madras by him. (*Madras Observations*, 1861, Oct. 4, p. 134.)

At the Hartwell Observatory, 1859 Jan. 1 to 1860 Dec. 31.

SL	3·8	60	As above.
H	5·9	102	Hartwell Equatorial.
Da	8·2	...	Mr Dawes' Equatorial at Haddenham.
B	10·0	144	Mr Barclay's Equatorial at Leyton.

At Madras, 1861 Feb. 8 onwards.

L	6·0	...	Lerebours Equatorial.
D	3·5	...	Dollond telescope.
S	8·0	...	Simms Equatorial.
SL	3·8	60	Smythian or Lee.

5. Many of the observations of U Geminorum in the first ten years are records of invisibility, with estimated superior limits; and to save space, these are collected in Table I.

TABLE I.

Dates in the Years 1856–1865 *when U Geminorum was looked for without success.*

Date.	Tel. Power.	Inferred.
1856.		
Jan. 2	E 54	<12
27	R 40	<11
Maximum here.		
Apr. 2	?	$<13{\cdot}5$
4	?	<12
10	?	<12
12	?	$<d+0{\cdot}5$
16	?	$<e+0{\cdot}5$
20	R	$<11{\cdot}0$
24	E	
May 10	,,	$<e+10$
13	,,	$<d$
20	,,	$<d$
22	,,	$<\begin{cases} f+1{\cdot}3 \\ e+1{\cdot}7 \end{cases}$
Sept. 3	R	$<11{\cdot}0$
Oct. 21	Dd	$<11{\cdot}0$
25	,,	$<11{\cdot}0$
Nov. 5	,,	$<11{\cdot}0$
8	,,	$<11{\cdot}0$
Nov. 26	E 93	$<13{\cdot}5$
29	,,	$<13{\cdot}5$
Dec. 1	,,	$<13{\cdot}5$
15	E 54	$<12{\cdot}0$
24	R 40	$<11{\cdot}0$
27	E 54	$<12{\cdot}7$
29	E 93	$<13{\cdot}5$
1857.		
Jan. 14	E 93	$<13{\cdot}0$
15	E 65	
16	,,	
27	,,	
29	,,	
31	,,	
Feb. 13	,,	
14	,,	
16	,,	
20	,,	
23	,,	
26	,,	

TABLE I.—*continued.*

Date.	Tel. Power.	Inferred.
1857.		
Feb. 28	E 65	
Mar. 3	,,	$<d$
4	E 95	$<f$
12	,,	$<13{\cdot}0$
13	,,	$<13{\cdot}0$
17	,,	$<13{\cdot}5$
20	,,	$<13{\cdot}5$
Maximum here.		
Apr. 18	E 95	$<13{\cdot}5$
May 5	E 65	$<11{\cdot}5$
Aug. 28	SL 90	$<11{\cdot}5$
Sept. 17	,,	$<12{\cdot}0$
18	,,	$<12{\cdot}6$
Oct. 1	SL 50	$<11{\cdot}0$
7	,,	$<11{\cdot}0$
8	,,	$<10{\cdot}7$
13	,,	$<12{\cdot}5$
16	,,	$<12{\cdot}4$
27	,,	$<12{\cdot}3$
Maximum here.		
Nov. 16	SL 50	$<12{\cdot}5$
18	E 95	$<13{\cdot}5$
28	E 54	$<13{\cdot}0$
Dec. 19	,,	$<13{\cdot}5$
1858.		
Jan. 9	SL 50	$<12\ 3$
11	,,	$<12{\cdot}5$
13	,,	$<12{\cdot}5$
18	,,	$<12{\cdot}5$
20	,,	$<12{\cdot}5$
21	,,	$<12{\cdot}5$
23	,,	$<12{\cdot}1$
25	,,	$<11{\cdot}5$
31	,,	$<11{\cdot}3$
Feb. 4	,,	$<12{\cdot}7$
7	,,	$<12{\cdot}5$
9	,,	$<12{\cdot}7$
15	,,	$<12{\cdot}1$
16	,,	$<12{\cdot}7$

Date.	Tel. Power.	Inferred.
1858.		
Feb. 19	SL 50	$<12{\cdot}5$
21	,,	$<11{\cdot}0$
28	,,	$<12{\cdot}3$
Mar. 5	,,	$<12{\cdot}7$
8	,,	$<12{\cdot}6$
9	E 96	$<13{\cdot}5$
11	SL 50	$<12{\cdot}5$
17	E 96	$<13{\cdot}5$
21	SL 50	$<11{\cdot}0$
22	E 54	$<11{\cdot}0$
26	,,	$<11{\cdot}2$
Apr. 1	SL 50	$<12{\cdot}0$
10	E 65	$<13{\cdot}6$
13	,,	$<13{\cdot}0$
15	,,	$<11{\cdot}0$
17	,,	$<11{\cdot}0$
23	,,	$>11{\cdot}5$
30	,,	$<13{\cdot}5$
May 3	SL 50	$<11{\cdot}5$
6	E 65	$<11{\cdot}0$
7	,,	$<12{\cdot}5$
11	,,	$<13{\cdot}0$
19	B 41	$<11{\cdot}3$
Sept. 12	SL 50	$<12{\cdot}5$
Oct. 8	E 65	$<13{\cdot}5$
Nov. 2	H 50	$<13{\cdot}5$
9	SL 74	$<12{\cdot}5$
Maximum here.		
Dec. 2	SL 100	$<12{\cdot}5$
26	SL 50	$<12{\cdot}0$
1859.		
Jan. 5	H 50	$<13{\cdot}5$
7	,,	$<13{\cdot}0$
12	,,	$<12{\cdot}5$
22	,,	$<13{\cdot}0$
26	,,	$<12{\cdot}1$
28	,,	$<13{\cdot}3$
Feb. 7	SL 50	$<12{\cdot}5$
Maximum here.		

TABLE I.—*continued.*

Date.	Tel. Power.	Inferred.
1859.		
Mar. 2	H 118	<13·5
7	H 50	<13·0
30	SL 50	<12·5
May 3	,,	<12·5
12	SL 74	<10·7
14	,,	<10·7
16	SL 50	<11·0
21	SL 74	<11·0
23	SL 50	<11·0
Aug. 29	H 66	<12·4
Sept. 4	SL 74	<12·1
Nov. 7	H 118	<13·0
21	H 50	<11·8
28	H 50	<13·0
30	H 118	<12·9
Dec. 1	,,	<13·0
3	,,	<13·0
5	,,	<12·9
8	,,	<12·2
14	,,	<13·0
21	,,	<13·0
26	H 84	<13·1
1860.		
Jan. 6	H 84	<12·3
16	,,	<13·0
20	,,	<13·0
27	,,	<13·0
31	,,	<12·8
Feb. 9	H 66	<12·6
13	H 50	<13·0
19	,,	<13·0
27	H 66	<12·7
29	H 50	<12·7
Mar. 9	SL 52	<11·3
15	SL 74	<12·3
18	H 50	<13·0
24	H 118	<13·0
26	H 66	<13·0
Apr. 2	H 118	<12·7
16	H 52	<12·0

Date.	Tel. Power.	Inferred.
1860.		
Maximum here.		
Aug. 22	H 50	<12·0
Sept. 12	H 52	<12·7
18	,,	<12·5
25	H 50	<12·5
30	SL 20	<10·5
Oct. 2	H 50	<11·0
3	H 50	<12·0
11	,,	<12·5
20	,,	<13·0
Nov. 1	H 50	<11·5
15	,,	<13·0
22	,,	<12·5
Dec. 20	B 40	<11·0
1861.		
Feb. 9	L 66	<13·0
13	,,	<13·0
14	D	<12·5
16	L 66	<13·5
28	,,	<13·0
Mar. 14	,,	<13·0
17	D	<12·0
18	,,	<12·3
22	L 66	<12·5
30	,,	<13·0
31	,,	<13·0
Apr. 1	,,	<13·0
2	,,	<13·0
5	,,	<13·0
9	D	<12·5
14	,,	<12·0
May 16	L 66	<10·5
Maximum here.		
Oct. 10	L 62	<10·5
27	SL 50	<12·4
29	L 62	<13·0
Nov. 15	L 77	<12·5
16	L 62	<12·5
18	,,	<12·5
23	,,	<12·5

TABLE I.—*continued.*

Date.	Tel. Power.	Inferred.
1861.		
Nov. 27	L 77	<13·0
Dec. 3	L 62	<13·2
6	SL 50	<12·7
9	L 77	<13·2
14	L 62	<12·8
17	,,	<12·8
23	L 77	<13·1
Maximum here.		
1862.		
Jan. 30	L 66	<13·0
Feb. 8	SL 50	<12·0
Mar. 22	,,	<12·5
27	L 66	<13·0
Apr. 4	,,	<13·0
9	,,	<12·5
17	,,	<12·9
23	,,	<12·5
24	,,	<13·0
May 1	,,	<13·0
20	,,	<13·0
23	,,	<13·0
Oct. 6	,,	<12·7
16	L 62	<12·7
Nov. 4	SL 74	<11·5
9	SL 52	<11·0
10	L 66	<11·7
25	,,	<13·0
Dec. 31	,,	<12·8
1863.		
Jan. 6	,,	<12·5
12	,,	<13·0
21	,,	<13·0
Feb. 6	,,	<12·8
12	,,	<13·0
22	,,	<13·0
Mar. 7	,,	<12·7
12	,,	<13·0
23	,,	<12·8
Maximum here.		
May 5	L 66	<13·0

Date.	Tel. Power.	Inferred.
1863.		
Oct. 7	SL 50	<12·5
28	L 66	<12·5
Nov. 11	,,	<13·0
22	,,	<12·8
26	,,	<12·5
Dec. 7	,,	<13·2
22	,,	<12·7
Maximum here.		
1864.		
Jan. 2	L 164	<13·5
25	L 63	<12·5
Feb. 15 to 29	,,	<13·0
	,,	<12·9
Mar. 5	,,	<12·8
May 25	,,	<13·0
Maximum here.		
Oct. 4	L 164	<13·0
Dec. 6	L 63	<13·0
Maximum here.		
1865.		
Jan. 14	L 63	<12·8
25	,,	<13·1
Feb. 4	SL 52	<12·3
14	SL 74	<12·0
25	SL 52	<12·5
Mar. 2	,,	<12·4
19	L 63	<12·7
21	,,	<13·0
24	,,	<13·0
Maximum here.		
Sept. 26	L 63	<12·7
30	L 70	<13·0
Oct. 10	L 63	<13·0
?	,,	<13·0
Nov. 4	,,	<12·7
10	L 164	<13·0
30	L 63	<12·8
Dec. ?	,,	<13·0
14	,,	<13·1
23	L 70	<13·3

6. *Comparison Stars.*—Pogson kept a small MS. book labelled "Comparison Stars for Variables," in which usually the upper half of the page is devoted to those for one star and the lower half to those of another. Thus p. 17 is shared by R Cancri and V Piscium. But the whole of the opposite page (16) is devoted to the stars for U Geminorum, and some of the records are squeezed in margins, having obviously been added. The stars first adopted were *a*, *b*, *d*, *e*, *f*, *g*; then *c*, *h*, *e*, *l*, *m* were added in the margins, and finally new measures of *e*, *f*, *g*, *h* and the star *n* were squeezed in. No dates are given of the separate observations. When ten measures of any star had been secured, the mean was taken, and the star was thenceforward denoted by this magnitude, omitting the decimal point. Thus star *a* is often called 88, even in the original notebooks; and where it is called *a* in the notebooks it is copied out as 88. Sometimes the adopted magnitude of a star has been changed, and thus the same star is designated by different numbers at different times. Thus the magnitudes of *b* and *c* were at first taken as 9·3, and both stars designated by this number. Later the revised magnitudes were found $b = 92$, $c = 94$; and ultimately they were again changed to 93. Hence *the record of the original notebook is given in Table III. whenever possible*; but in some cases a doubt is inevitable. The original notebooks are not available before 1859. Search has been made for them, by the kind permission of the Radcliffe observer, at the Radcliffe Observatory, but so far without success.

There are some curious corrections and deletions, notably in the cases of stars *e*, *g*, and *h*, where ten large readings have been systematically struck out; but there seems to be no reason to restore them against Pogson's own judgment, though they are added at the end of the table. Those for *e* all come between the first and second series retained: those for *g* and *h* are interspersed among the separate members of the first set of ten retained.

TABLE II.

Pogson's Observations of Comparison Stars.

	a		*b*		*c*		*d*		*e*		*f*		
	9·0	9·0	9·3	9·5	9·5	9·5	10·2	10·2	11·1	10·8	11·4	11·3	11·3
	8·9	8·7	9·4	9·2	9·5	9·2	10·2	10·4	10·8	10·8	11·0	11·3	11·1
	8·7	9·0	9·1	9·6	9·0	9·5	10·2	10·4	11·0	10·7	11·3	11·2	11·4
	8·9	8·7	9·3	9·0	9·4	9·3	10·4	10·5	10·9	10·8	11·4	11·4	11·3
	9·0	8·8	9·1	9·2	9·5	9·5	10·5	10·3	10·6	10·9	11·0	11·2	11·3
	8·6	8·8	9·2	9·2	9·3	9·2	10·1	10·6	10·5	11·0	11·0	11·3	11·4
	8·6	8·7	9·2	9·2	9·3	9·1	10·3	10·5	10·9	10·6	11·3	11·5	11·3
	9·0	8·7	9·1	9·1	9·5	9·0	10·2	10·3	11·0	10·8	11·6	11·4	11·1
	8·7	8·5	9·2	9·4	9·2	9·4	10·1	10·5	10·7	10·6	11·2	11·2	11·1
	9·0	8·5	9·5	9·4	9·5	9·5	10·4	10·4	10·9	10·7	11·3	11·4	11·6
Means	8·84	8·74	9·24	9·28	9·37	9·32	10·26	10·41	10·84	10·77	11·25	11·32	11·29
Adopted	8·8		9·3		9·3		10·3		10·8		11·3		

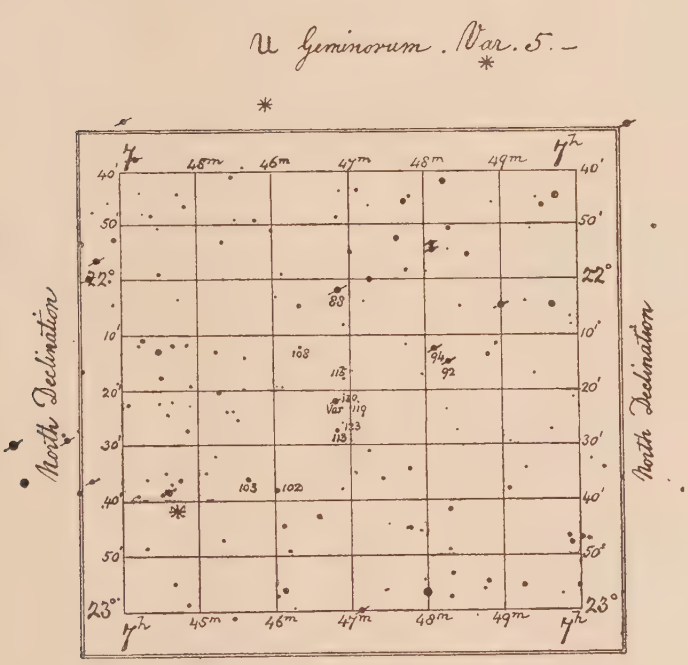
Readings struck out.

	g		*h*		*l*	*m*	*n*	*e*	*g*	*h*
	12·0	11·7	12·3	11·8	13·0	11·2	10·0	11·1	12·5	12·6
	12·0	11·6	12·2	12·5	13·0	11·3	10·1	10·8	12·1	12·5
	12·0	11·5	12·5	12·0	13·0	11·3	10·3	10·9	12·3	12·6
	12·0	12·0	12·2	12·4	13·0	11·5	10·2	11·1	12·2	12·5
	12·0	11·9	12·4	12·2	13·1	11·2	10·2	11·5	12·2	12·6
	12·0	11·9	12·0	12·3	13·1	11·3	10·3	11·1	12·3	12·6
	11·7	11·9	12·0	12·3	13·0	11·5	10·1	11·2	12·3	12·5
	12·0	12·0	12·4	12·5	12·9	11·4	10·2	11·0	12·6	12·6
	11·9	11·8	12·1	12·1	13·0	11·4	10·2	11·1	12·1	12·6
	12·0	11·9	12·3	12·5	12·9	11·3	10·1	11·1	12·5	12·5
Means	11·96	11·82	12·24	12·26	13·00	11·34	10·17	11·08	12·31	12·56
Adopted	11·9		12·3		13·0	11·3	10·2	...	...	...

7. From examination of Table II. the following identifications are suggested for the numbers by which Pogson indicates stars :—

88 or 89 = *a*; 92 = *b*; 93 = *b* or *c*;
94 = *c*; 102 = *n*; 103 = *d*; 108, 110, or 111 = *e*;
112 = *f*; 113 = *f* or *m*; 115 = *m*; 119, 120, or 121 = *g*;
123, 124, or 126 = *h*; 130 or 131 = *l*; 137 = ?

Pogson's diagram is reproduced with this paper, and the approximate positions given below are read from it. On reference to Mr Knott's diagram on p. 94 of *Memoirs R.A.S.*, vol. lii., it will be seen that he used practically the same stars, and a comparative table is given below.



Pogson. Diagram. No.	R.A.	Dec.	Letter.	Knott. Letter.	Mag.	R.A.	Dec.
	h m s	° ′				h m s	° ′
Var.	7 46 48	22 22	U	U	...	7 47 5	22 20
88	46 49	22 2	*a*	*a*	8·6	7 47 18	22 1
92	48 19	22 15	*b*	*c*	9·2	7 48 31	22 13
94	48 8	22 13	*c*	*b*	9·3	7 48 25	22 11
103	45 38	22 37	*d*	*d*	10·3	7 45 53	22 34
108	46 22	22 13	*e*	*e*	10·6	7 46 38	22 10
113	46 48	22 28	*f*	*f*	11·2	7 47 3	22 26
119	47 6	22 23	*g*	*g*	12·3	7 47 22	22 20
123	46 53	22 27	*h*	*h*	12·3	7 47 12	22 24
130	46 53	22 21	*l*	*k*	13·3	7 47 10	22 18
113	46 52	22 17	*m*	...	...	...	...
102	46 2	22 38	*n*	...	...	...	...
137	?	?	*k*?	*l*	13·7	7 47 0	22 22

8. This being premised, it seems the best course now to give simply *the original notebook record*, and this has been done in Table III. below. After 1865 the occasions when the star was looked for without success are comparatively few, and have been given in the ordinary form.

TABLE III.

Observations of U Geminorum near maximum 1856–1865; *and all observations* 1866–1881.

Date.	Tel.	Power.	Comparisons.
1856.			
Mar. 26	E	?	Est. 9·7 = 88 + 7 = 93 + 3 = 93 + 2
27	E	54	88 + 13 = 93 + 8 = 93 + 6 = 113 − 6
29	E	54	Est. 11·0 = 93 + 17
1857.			
Apr. 7	E	95	88 + 8 = 93 + 5 = 103 − 6
8	E	54	88 + 8 = 93 + 5 = 103 − 5
11	E	54	88 + 6 = 93 + 3 = 103 − 7
1857.			
Apr. 14	E	54	103 + 7 = 110 − 4 = 113 − 16
15	E	95	110 + 3 = 113 + 2 = 121 − 7 = 124 − 10
16	E	95	121 = 124 − 3
18	E	95	Invis. = < 13·5
Oct. 30	SL	50	9·7 ...
1858.			
Nov. 17	SL	50	93 + 5 = 103 − 3
18	SL	50	103 + 7 = 110 − 3 = 113 − 5

TABLE III.—*continued.*

Date.	Tel.	Power.	Comparisons.
1859.			
Feb. 16	H	50	$a+6=b=c-2=d-8$
17	SL	85	$a+3=b-2=c-2$
22	H	50	$a+2=b-3=c$
23	H	50	$b+3=c+1=d-7$
24	H	50	$b+6=c+5=d-4$
25	H	50	$c+15=d+6=f-4$
27	H	118	$f+10=g-1=h-4$
1860.			
Apr. 22	SL	50	$88+2=93-3$
24	SL	50	$a+3=b+2=c-2$
26	Da	66	$88+6=93$
27	SL	50	$88+10=93+5$
28	SL	50	$93+8$
30	H	52	$103+7=110-2$
May 1	H	52	$113+5=121-4$
2	Da	?	$121+4=130-7$
1861.			
May 2	L	66	$88+1=92-2$
3	L	66	$88+7=92+5=103-13$
4	L	66	$88+4=92$
5	L	66	$88+4=93$
6	L	66	$88+8=93+6$
7	L	66	$88+7=92+5$
8	L	66	$88+5=93+2$
9	L	66	$88+9=93+6$
10	L	66	$88+11=93+8=92+3$
12	L	66	$88+15=92+13=93+7=103-7$
14	L	66	$103+3=110-5$
16	L	66	Invis. $=<10\cdot5$
Dec. 23	L	77	Invis. $=<13\cdot1$
29	L	62	$103+2=110-5$
31	SL	50	$88+5=93-2$

Date.	Tel.	Power.	Comparisons.
1862.			
Jan. 2	SL	50	$88+2=93-3$
4	SL	50	$88+5=93-1$
5	SL	50	$88+7=93+1=103-5$
6	SL	50	$88+10=93+3=103-6$
8	L	62	$93+6=103-2$
10	SL	50	$103+2=110-3$
11	SL	50	$110-1=113-4$
18	L	77	$130+2$
1863.			
Apr. 8	L	66	$94+2=103-8$
9	L	66	$93+7=103-4$
10	L	66	$94+6=103-7$
11	L	66	$88+12=92+8=103-4$
12	L	66	$94+11=103-1=110-9$
13	L	66	$103+4=110-2$
14	L	66	$110+6=113+2=124-6$
15	L	66	$124+8=130-2$
17	L	164	$137+1$
Dec. 31	L	66	$124+2=130-4$
1864.			
Sept. 27	L	164	$110+6=112+2=121-5$
28	L	164	$121-1=124-5=113+7$
30	L	164	$130-1$
Oct. 4	L	164	Invis. $=<13\cdot1$
1865.			
Jan. 9*	L	63	$103+1=110-7$
14*	L	63	Invis. $=<12\cdot8$

* On opposite page of notebook is written "Looked in vain for U Geminorum on Jan. 12 and 13 (1865), but the moon was too bright to see even the well-known adjacent small stars." Is not observation of Jan. 9 possibly a wrong object?

TABLE III.—*continued.*

Date.	Tel.	Power.	Comparisons.
1865.			
Apr. 18	L	70	130 − 2
20	L	70	94 + 2 = 103 − 8
21	L	63	94 + 6 = 103 − 5
22	L	63	94 + 9 = 103 − 4
23	L	63	94 + 14 = 103 + 1 = 110 − 4
25	L	63	124 + 5 = 130 − 2
26	L	63	130 + 5
1866.			
Jan. 4	L	63	Invis. = < 12·5
14	L	70	Invis. = < 13·0
17	L	63	88 + 5 = 94 − 1 = 92 + 2
18	L	63	94 + 2 = 103 − 10
19	L	63	88 + 5 = 92 + 2 = 94 − 1
20	L	63	92 + 3 = 94 + 1 = 103 − 11
21	L	63	94 + 3 = 103 − 8
22	L	63	94 + 3 = 103 − 8
23	L	63	94 + 4 = 103 − 8
24	L	63	94 + 5 = 103 − 7
25	L	63	94 + 5 = 103 − 8
26	L	63	94 + 5 = 103 − 6
27	L	63	94 + 7 = 103 − 4
28	L	70	94 + 7 = 103 − 3
31	L	70	124 + 6 = 131 − 2
Feb. 1	L	164	Invis. = < 13·0
2	L	164	130 + 3 = 137 − 3
Mar. 21	L	63	Invis. = < 12·5
Apr. 6	L	63	Invis. = < 13·0
{ 16	L	70	h + 8 = 130 − 4
{ 16	L	70	h + 3 = 130 − 7
17	L	63	94 + 4 = 103 − 5
18	L	63	94 + 5 = 103 − 5
19	L	63	94 + 7 = 103 − 3 = 108 − 10
20	L	63	94 + 11 = 103 + 2 = 108 − 4
21	L	70	108 + 5 = 112 + 2 = 115 − 3

Date.	Tel.	Power.	Comparisons.
1866.			
Apr. 22	L	70	124 + 4
23	L	164	Invis. = < 12·8
May 14	63	63	Invis. = < 12·8
Nov. 3	S	45	Invis. = < 13·0
Dec. 25	S	86	Invis. = < 13·5
1867.			
Jan. 17	L	63	Invis. = < 13·0
23	L	63	Invis. = < 13·0
Mar. 8	S	86	Invis. = < 13·5
Oct. 26	L	63	Invis. = < 13·0
Dec. 14	S	86	88 + 10 = 93 + 5 = 103 − 6
15	S	40	93 + 5 = 103 − 8 = 88 + 10
16	S	40	93 + 7 = 103 − 3
17	S	86	n − 1 = 103 − 2
18	S	86	108 = m − 5
19	S	86	113 + 3 = 112 + 1 = 119 − 7
20	S	86	h + 1 = 130 − 6
21	S	86	130 + 1
22	S	86	130 + 5 = 137 − 1
23	S	163	equal 137
1868.			
Jan. 21	S	86	Invis. = < 13·7
Mar. 11	S	86	Invis. = < 13·0
Apr. 9	S	86	Invis. = < 13·7
18	S	86	Invis. = < 13·5
Nov. 17	S	105	137 + 3
18	S	86	137 + 3
Dec. 9	S	86	Invis. = < 13·5
1869.			
Jan. 6	L	66	Invis. = < 13·0
Feb. 20	S	86	93 + 4 = 102 − 7
21	S	86	93 + 5 = 102 − 4
23	S	86	93 + 7 = 102 − 4
24	S	86	102 + 4 = 108 − 2
27	S	102	< 12·8
Oct. 30	S	86	Invis. = < 14·0

TABLE III.—*continued.*

Date.	Tel.	Power.	Comparisons.
1870.			
Jan. 17	S	86	94+2=103−5
19	S	86	94+4=103−7
22	S	86	94+5=103−4
23	S	86	103−1
25	S	86	103+7=108+4 =113−2
26	S	86	119+3=123
27	S	86	130+1
Nov. 24	S	86	Invis. = <13·7
1872.			
Jan. 17	S	105	Invis. = <13·5
22	L	66	Invis. = <12·5
27	L	166	Invis. = <12·7
Feb. 1	L	166	Invis. = <13·7
6	L	66	Invis. = <13·5
11	L	66	Invis. = <13·7
16	S	...	Invis. = <14·0
21	S	105	93+15=103+2 =113−9
22	S	45	89+5=93
23	S	45	89+3=92
24	S	45	89+3=92−2 =94−4
25	S	45	89+5=92+2 =94
26	S	45	92+4=94+2 =103−9
27	S	45	92+3=94+2 =103−8
28	S	45	92+3=94+4 =103−8
29	S	45	92+6=94+4 =103−7
Mar. 1	S	45	92+7=94+6 =103−6
2	S	45	92+8=94+7 =103−4
3	S	45	92+12=94+10 =103−3
4	S	45	93+12=103−2 =108−6
5	S	45	103+2=108−4
6	S	90	108+4=113+1

Date.	Tel.	Power.	Comparisons.
1872.			
Mar. 7	S	90	123+3=130−6
8	S	90	130+2=138−6
9	S	90	=138
10	S	90	Suspected = <14·0
12	L	63	Invis. = <13·0
1873.			
Feb. 15	L	53	Invis. = <12·5
Mar. 27	L	53	Invis. = <12·7
1874.			
Jan. 14	L	106	Invis. = <13·0
Feb. 18	L	63	Invis. = <13·0
1875.			
Jan. 15	L	63	88+8=94+4 =102−7
16	L	63	88+10=94+4 =103−7
18	S	73	88+10=94+5 =102−4
19	L	63	88+12=94+5 =102−4
20	S	73	94+6=102−3
22	S	73	94+7=102−3
24	S	73	108+1=113−3
26	S	102	123+6=130−2
27	S	102	130+2=137−7
Feb. 1	S	99	130+7=137−2
?	S	120	<14·5 (suspected)
Dec. 18	L	67	Invis. = <13·0
1876.			
Jan. 13	S	89	Invis. = <13·0
1877.			
Jan. 4	L	61	Invis. = <12·7
Mar. 11	S	73	Invis. = <13·5
May 9	L	61	Invis. = <13·0
Oct. 13	S	73	Invis. = <13·0
1880.			
Jan. 2	S	72	Invis. = <13·0
1881.			
Mar. 23	L	65	Invis. = <12·7

Stellar Parallax Papers, No. 3.

The Parallax of Eight Stars, from Photographs taken at the Cambridge Observatory by Arthur R. Hinks, M.A., and the writer. By Henry Norris Russell, Ph.D.

The following results are derived from plates taken at Cambridge by Mr A. R. Hinks and the writer, and measured and discussed by the latter in the course of his work as a research assistant of the Carnegie Institution. A full description of the methods of observation and reduction is given on pp. 775–800 of the *Monthly Notices* for June 1905.

Table I. gives the relative parallax of these stars with respect to comparison stars averaging about the 9th magnitude. The last column but one gives the number of comparison stars for each series, and the preceding column the number of plates in the series. The same comparison stars were used for Nos. 2 and 3, which appear on the same plates, and similarly for Nos. 7 and 8. The two bright stars β and η Cassiopeiæ were photographed with a colour-screen, which reduced their photographic brightness by about $5\frac{1}{2}$ magnitudes.

Table I.

Ref. No.	Star.	R.A. 1900.0 h. m.	Dec. 1900.0 ° ′	Mag.	P.M. ″	Parallax. ″	Plates.	Comp. Stars.	r_1. ″
1	β Cassiopeiæ	0 3·8	+58 26	2·4	0·55	+0·082±0·009	5	9	±0·014
2	Groombridge 34	0 12·6	+43 27	7·9	2·82	+0·250±0·012	6	9	±0·019
3	26 Andromedæ	0 13·5	+43 15	5·9	0·03	−0·026±0·041	6	9	±0·069
4	η Cassiopeiæ	0 42·9	+57 18	3·6	1·20	+0·188±0·021	7	8	±0·041
5	o Ceti	2 14·3	− 3 26	Var.	0·24	+0·136±0·035	7	9	±0·070
6	Lalande 25372	13 40·7	+15 27	8·5	2·32	+0·221±0·019	8	9	±0·032
7	Berlin B 5072	14 21·1	+24 6	9·0	1·42	+0·067±0·040	7	7	±0·073
8	Berlin B 5073	14 21·1	+24 6	9·1	1·42	+0·000±0·029	7	7	±0·052

The last column gives the probable error of a co-ordinate of each star, resulting from one plate, as derived from the least-square solution for the parallax. If we divide the stars into three classes according to their effective photographic brightness, we have three stars with faint images, Nos. 5, 7, and 8, four of moderate brightness, Nos. 1, 2, 4, and 6, and one star, No. 5, whose images are somewhat large and diffuse. The average values of the probable error of one plate for these three groups are ±0″·065 for the faint stars, ±0″·027 for those of medium brightness, and ±0″·069 for the bright star.

This emphasises the importance of proper exposure when we wish to secure highly accurate plates. It would have been well if the exposures for the three faint stars, which were made of the usual length (five minutes), had been lengthened.

Stars brighter than the 6th magnitude were, as a rule, excluded from our working list, unless observed with a colour-screen. The large probable error for No. 3 (which was only measured because it happened to be on the plates of No. 2) justifies this course, while the two good results obtained with the colour-screen show that it affords a satisfactory way of avoiding the over-exposure difficulty.

Several of these stars have already been observed for parallax. The results, so far as known to the writer, are as follows :—

β Cassiopeiæ.

$\pi = +0''.15 \pm 0''.02$	Pritchard, photography.
$\pi = +0''.14 \pm 0''.03$	Kostinsky, absolute declinations.
$\pi = +0''.10 \pm 0''.03$	Flint, meridian transits.

Groombridge 34.

$\pi = +0''.29 \pm 0''.025$	Auwers, diff. of R.A., micrometer.
$\pi = +0''.31 \pm 0''.034$	Flint, transits.

η Cassiopeiæ.

$\pi = +0''.10 \pm 0''.05$	from distances } O. Struve,
$\pi = +0''.37 \pm 0''.10$	,, pos. angles } filar micrometer.
$\pi = +0''.20 \pm 0''.06$	from distances } Schweizer,
$\pi = +0''.14 \pm 0''.08$	,, pos. angles } filar micrometer.
$\pi = +0''.44 \pm 0''.04$	Davis, from Rutherfurd photographs.
$\pi = +0''.34 \pm 0''.04$	Flint, transits.
$\pi = +0''.18 \pm 0''.010$	Peters, heliometer.

Lalande 25372.

$\pi = +0''.43 \pm 0''.065$	Flint, transits.
$\pi = +0''.17 \pm 0''.043$	Elkin, heliometer.

Flint's results have received large corrections for systematic personal error, and the Rutherfurd photographs of η Cassiopeiæ, being taken at widely different hour angles, are affected to a considerable but unknown degree by atmospheric dispersion. If we reject the last, give Flint's values half weight, and the heliometer results double weight, we obtain mean values for the parallax of the four stars, from which the results of the present paper differ by $0''.05$, $0''.05$, $0''.00$ and $0''.00$ respectively.

If we assume that our results are responsible for two-thirds of the discrepancies, their average probable error would be $\pm 0''.020$, including the effects of any systematic error. It would therefore appear that the latter must be very small.

Examination of the residuals for the comparison stars shows no conspicuous evidence of parallax or proper motion except for B.D. $+43^\circ$ 55 mag. 9·2 which appears on the plates of No. 2, and seems to have a proper motion of about $+0''.3$ in x. By Kapteyn's formula, the parallax of this star should be about

0″·029, while the average parallax of stars of the ninth magnitude is 0″·006.

We may therefore assume that the mean parallax of our comparison stars is 0″·009 for No. 2, and 0″·006 for the rest of our series. Adding this to the relative parallaxes of Table I., we obtain from the resulting absolute parallaxes the following values of the absolute magnitudes of the stars (*i.e.* their magnitudes if at such a distance that their parallax was 0″·10) and of their light in terms of the Sun's, and their velocities at right angles to the line of sight.

Groombridge 34 and η Cassiopeiæ are double, and data are given for both components, and for Mira at maximum (mag. 3·5) and minimum (9·5).

TABLE II.

No.	Star.		Absolute Mag.	Light.	Cross-velocity. Ast. units per year.	Cross-velocity. Kilometres per sec.
1	β Cassiopeiæ		2·1	23·	6·2	29
2	Groombridge 34	A	10·0	0·016	10·9	52
		B	12·6	0·0015		
4	η Cassiopeiæ	A	5·1	1·5	6·2	29
		B	9·1	0·04		
5	o Ceti	Max.	4·3	3·0	1·7	8
		Min.	10·3	0·011		
6	Lalande 25372		10·3	0·011	10·2	49
7	Berlin B 5072		(7·0)	(0·25)	(35)	(170)
8	Berlin B 5073		(7·1)	(0·23)		

Nos. 7 and 8, which are 45″ apart, have a common proper motion,* and are no doubt physically connected. The mean of the observed parallaxes, $+0''\cdot033 \pm 0''\cdot025$, has therefore been taken as the true value for both stars, but it is clear that little reliance can be placed upon the numbers calculated from it. It is, however, probable that these stars are fainter than the Sun, and are moving across the line of sight faster than any of the others in the table.

The negative result for 26 Andromedæ is less than its probable error, and indicates that the parallax of this star is insensible. It was measured because the Bonn A. G. catalogue gives its proper motion in R.A. as $+0^{s}\cdot016$, which is ten times the true value. If its actual cross-velocity is equal to the mean of that of the other stars (excluding Nos. 7 and 8), its parallax would be only 0″·004. It is double (OΣ 5), and its 10th magnitude companion is relatively fixed in 240°, 6″·1—which should be the case if its parallax is so small.

η Cassiopeiæ is a well-known binary. From Lewis's orbit †

* Bellamy, *M. N.*, Dec. 1899, p. 126.

† *Mem. R.A.S.*, vol. lvi. p. 16.

we have $a = 8''.51$ P = 233 years, which with our parallax gives the mean distance of the components as 44 astronomical units. The actual distance varies from 58 to 30, and the combined mass is 1·6 times that of the Sun. According to Lewis, the bright star is twice as massive as the fainter one, so that it very nearly equals the Sun in mass as well as in light.

Groombridge 34 has a physical companion, which was measured by Auwers * in 1865, and is shown on some of the plates of the present series, as is also a fainter companion nearer the principal star.

The writer's measures, made this month with the 23-inch equatorial of the Halsted Observatory at Princeton, show that this is only an optical companion. They give, for the co-ordinates of the companions, relative to the principal star,

		B $10^m.5$		C $11^m.5$	
		x	y	x	y
Auwers	1866·23	+31″·2	+23″·4		
Plate 391	1904·98	+32·3	+21·6	+32″·0	−13″·3
Halsted Obs.	1906·78	+32·2	+21·5	+26·6	−14·2
		Proper motion of A in interval		+ 5·1	+ 0·7

This companion affords an unusually good opportunity for the micrometric determination of the parallax of the other two stars.

The actual separation of A B is at least 150 astronomical units —four times that of η Cassiopeiæ—so that we may expect the period of the system to be some thousands of years.

The results for Mira are also of interest. If the other long-period variables resemble it, and their maximum rather than their minimum light is comparable with the Sun's, the brighter ones must have easily measurable parallaxes, and it would pay to observe them.

Campbell and Stebbins † find that the radial velocity of Mira is constant, and equal to +63 km. per sec. As its cross-velocity is only 8 km., it is moving almost directly away from us—in a direction making an angle of only 7° with the line of sight.

If the present value of the parallax is correct, it follows that Mira was nearest the Sun about 110,000 years ago. It was then in Ursa Major, and had a parallax of 1″·1 and a proper motion of 15″ per annum. If its intrinsic brightness varied between the same limits as at present, it was of the 5th magnitude at minimum, and at maximum was as bright as Sirius.

Hearty thanks are due to Professor Lovett of Princeton for enabling the writer to continue the work here.

* *Math. Abth. Berlin Akad. der Wissenschaften*, 1867, p. 23.
† *Astrophysical Journal*, vol. 18, p. 341.

Princeton University Observatory:
1906 *October* 20.

Hansteen's Eclipse at Stiklastad, 1030 *August* 31.

By P. H. Cowell.

The record states that the eclipse occurred during the battle at Stiklastad at which Olaf the Fat was killed, and the date assigned to the battle is 1030 July 29. There are two reasons for supposing this date to be in error: the narrative mentions a dark night following the battle, whereas on July 29 the sun never sinks as much as 9° below the horizon of Stiklastad; also, Olaf was canonised, and July 29 became his festival and in time the supposed day of his death. Now August 31 was already at that time assigned to another saint. The foregoing arguments are Hansteen's (*Ast. Nach. Ergänzungsheft*, p. 49). Dr Dreyer (*The Observatory*, 1895 October, p. 363), quoting Prof. Konrad Maurer, decides against the later date for the battle. His arguments, as far as they are based on the day of the week, do not appear to me conclusive, as the chronicler may easily have counted backwards. But in any case we have a description of a total eclipse, that it was beyond the power of the chronicler to invent, and the eclipse is stated to have occurred at Stiklastad.

The eclipse has therefore been worked up. Considering its comparatively recent epoch, it might have turned out not to discriminate between the present tables and my formulæ. The very contrary is the case. My formulæ leave 3″ of the northern end of the Sun's diameter uncovered (indicating the possibility of small errors that I am quite prepared to admit). Hansen's tables and the present tables, which are practically the same for the epoch 1030, shift the Moon 35″ further south relatively to the Sun. The low altitude of the Sun makes this correspond to about 100 miles on the Earth's surface.

Outline of Calculations.

$T = -7^{d}\cdot69303 =$ 1030 Aug. 31 + 28°·53 G.M.T.

$g = 286^{\circ}\ 12'\ 41''\cdot8$	$L' = 165^{\circ}\ 0'\ 9''\cdot8$
$\omega = 88\ 35\ 37''\cdot2$	$\pi' = 266\ 18\ 7''\cdot1$
$-\Omega = 207\ 10\ 12''\cdot3$	

Inequalities of Moon's Longitude:—

A. Solar terms over 20″	$-16\ 752''\cdot7$
B. ,,	$1''\cdot35 - 69''\cdot8$
C. ,,	$0''\cdot15 - 5''\cdot0$
Figure of Earth terms	$+ 4''\cdot9$

Moon's Latitude and Sine Parallax:—

A. Solar terms over 10″	$+3\ 089''\cdot5$	$+3\ 500''\cdot9$
B. ,,	$0''\cdot55 + 27''\cdot0$	$+ 0''\cdot1$
C. ,,	$0''\cdot05 - 1''\cdot0$	$- 0''\cdot1$
Figure of Earth terms	$- 3''\cdot9$	

Movements in Julian century $\div 10^6$:—

In geocentric elongation in longitude $1681''.6$
In geocentric latitude $165''.3$
Stiklastad $11^\circ\ 35'$ E; $63^\circ\ 38'.8$ geocentric latitude; $1-\rho = 0.00268$
Parallax in longitude $+210''.7$; in latitude $-3122''.8$

Movements in Julian century $\div 10^6$ as seen from Stiklastad:—

In elongation in longitude $1480''.3$
In latitude $44''.5$

Hence if $0.09\Delta F$, ΔD are the corrections required by the latitude and difference of longitudes, then the latitude at apparent conjunction in longitude is

$$-8'' + 0.09\Delta F - 0.03\Delta D$$

The difference of apparent semi-diameters is about $5''$.

Note on the Approaching Return of Halley's Comet.
By A. C. D. Crommelin.

It is well known that Dr A. J. Ångström published in 1862 a paper entitled "Sur deux inégalités d'une grandeur remarquable dans les apparitions de la comète de Halley" (Actes de la Société royale des Sciences d'Upsal, Sér. III., t. iv.). In this paper he discusses the observed perihelion passages, as determined by Dr Hind, from B.C. 11 to A.D. 1835, and deduces the mean period of the comet, 76·93 years, this period being affected by two large inequalities of amplitudes 1·5 years, 2·3 years, periods 2650 years, 782 years. He has found theoretical arguments which will satisfy these periods, viz. $13\,\mathcal{E} - 2\,\mathcal{U}$, $\mathcal{U} + \mathcal{H} - 9\,\mathcal{E}$, where the symbols denote the mean annual motions of comet, Jupiter, Saturn. The amplitudes have been obtained by observation, and in no case does the error in the formula as compared with the observed time of perihelion exceed 1 year; in most cases it is less than half a year. Dr Ångström does not claim that these two are the only inequalities, merely that they are the most important ones.

I do not think it is so generally known that the time of the next perihelion passage, as deduced from Dr Ångström's curve, is altogether different from that published by Count de Pontécoulant. I deduce from the curve 1913·08 for the time of the next passage, whereas that given by Count de Pontécoulant is 1910·37, a discordance of 2·7 years.

We are not, of course, justified in assuming that M. Pontécoulant's

result is erroneous, solely because it differs from Dr Ångström's curve. For exact numerical computation is entitled to far more weight than an empirical method. But it is difficult not to feel some slight uneasiness about the matter, when we consider the extreme length and intricacy of the calculations, and the possibility that some important numerical error may have escaped detection. It is surely a *desideratum* that the perturbations should be independently computed, and I hope that some mathematicians may be actually engaged in the work; if not, there is still time for someone with the necessary leisure and ability to undertake it. Before the 1835 return there were at least five independent computations of the orbit—those of Damoiseau, Pontécoulant, Lehmann, Rosenberger and Lubbock; and it is difficult to understand why an equal amount of interest is not shown in the approaching return. Pontécoulant's result was published in 1864, and doubtless he regarded it as certain that there would be numerous investigations when the time drew nearer, so that he may well have given somewhat less attention to the next return than he gave to that of 1835. This is borne out by the fact that there are certainly some slips or misprints (not having seen his manuscript, I cannot say which) in his paper as printed in *Comptes Rendus de l'Académie*, tome lviii. Thus on p. 826 he gives T as 1910 May 24·37, Paris civil time (I have verified that this is the correct value from the figures and formula that he quotes), but on p. 828 T is given as May 16·95; this is merely an error of copying, and is corrected in a note in a subsequent number of *Comptes Rendus* (p. 915 of the same volume). The *Connaissance des Temps*, however, quotes the latter result converted into astronomical reckoning; while Dr Hind (*Encyc. Brit.*, 9th edit., vol. vi. p. 193) quotes the former, which is unquestionably the right one to take. Another case is that of the perturbation in the eccentricity. This is made up of three terms due to Jupiter, Saturn, and Uranus, the separate terms and their sum being given as follows on p. 827:—

Planètes.	Altération de l'excentricité $\int de$.
♃	− ·00529752
♄	− ·00110485 0
♅	+ ·000040625
Total,	− ·005655833

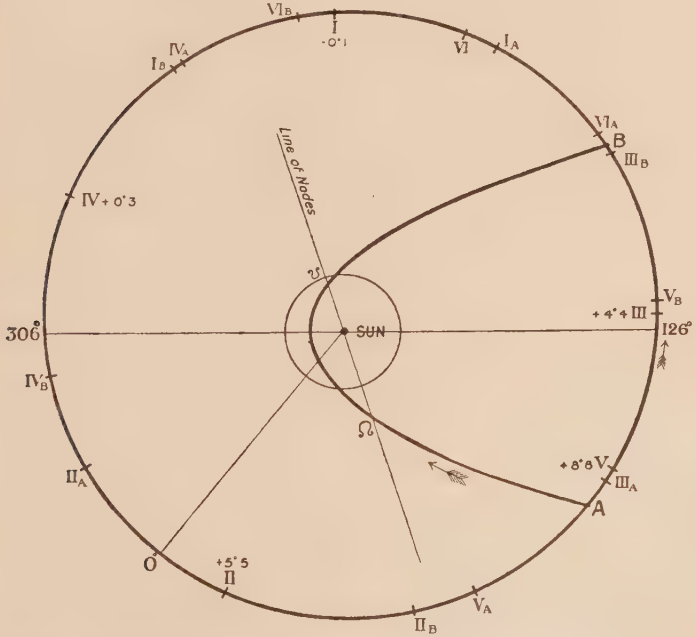
It is at once obvious that the total does not agree with the separate terms as printed, nor am I able to suggest any conjectural emendation which will bring them into harmony.

This value of the total is used in deducing the eccentricity as given in the elements for 1910, and it is noteworthy that both the eccentricity and perihelion distance for that epoch show a striking discordance from those in earlier apparitions, as this table shows.

Year.	Eccentricity.	Perihelion Distance.
1531	0·9684	0·568
1607	0·9669	0·584
1682	0·9677	0·583
1759	0·9676	0·5845
1835	0·9674	0·5866
1910	0·9617	0·6872

The change in the distance is a full tenth of the Earth's distance from the Sun. I do not assert that such a change is impossible, but it is certainly desirable that it should be verified, as I strongly suspect that there is a confusion in the position of the decimal point, and that the actual change is only $\frac{1}{10}$ of that given.

I have prepared a diagram showing the portion of the comet's orbit inside the orbit of Jupiter. The numerals I., II., III., IV., V., VI., indicate the positions of Jupiter at the instant of the comet's perihelion passage in 1531, 1607, 1682, 1759, 1835, 1910, while the same numerals with suffixes A, B indicate Jupiter's positions when the comet passed the points A and B. The distances of A, B from Jupiter's orbit are 1·0, 1·7 respectively. The



Orbits of Jupiter, Earth, Halley's Comet, showing positions of Jupiter, 1531, 1607, 1682, 1759, 1835, 1910.

numbers near the numerals I., II., etc., indicate the Jovian perturbation of the comet's mean anomaly in the revolution following that passage. It will at once be seen that the figure opposite V. is abnormally large, although the approach to Jupiter was less close than for passage III. This increases the suspicion that the perturbations for the present revolution may be too large; indeed, in the case of the perturbation in eccentricity the suspicion rises almost to a certainty.

I do not know whether Pontécoulant's calculations are still in existence; the article in *Comptes Rendus*, vol. lviii., does not mention where they are to be found. Assuming that they are still in existence, a careful re-examination of all the figures might suffice instead of an entirely independent computation of the perturbations. For there is no question whatever of Pontécoulant's ability, merely a suspicion of possible numerical slips. The inconsistency that I have adduced above is of itself sufficient to demand such a re-examination, before the results can be received with confidence. One can hardly imagine a greater loss of prestige to astronomy than that which would arise if there were a notable error in the prediction of this return of the comet, after the wonderful success achieved in 1759 and 1835.

Assuming that the date 1910 May is correct for the perihelion passage, I think the complete failure of the Ångström curve is not without a warning to us. We have here a curve which admirably fits 25 successive passages, and yet the first time it is used to predict a return it breaks down utterly, the error being almost 3 years, or three times the largest previous error. This is a decidedly startling fact, and indicates the danger of using terms of an empirical nature in lunar or planetary tables. Where such are used, it is at least desirable that means should be provided for readily effecting their removal from the calculated positions.

The Proper Motion of Castor. By A. C. D. Crommelin.

In the determinations that have hitherto been made of the proper motion of Castor, it does not appear that any effort has been made to separate true proper motion from orbital motion; the values given have merely been deduced from the meridian observations of the principal star, from the time of Bradley to the present day, on the assumption of uniform rectilinear motion. This assumption was fairly justifiable for the 130 years from 1755 to 1885, during which the curvature of the orbital path was slight; it is now rapidly increasing, so that the orbital motion in 1906 is at right angles to that in 1820, and in a few years the accepted proper motion will become entirely erroneous. There is as yet scarcely a long enough arc covered by the meridian observations to make a reliable determination of the relative masses from them. An attempt is made by Mr Lewis and Mr Furner in

the recently published Memoir on the Struve Double Stars (vol. lvi. p. 215); but while the collection of meridian observations and their reduction to a common epoch is of great interest and value, it seems to me that their determination of the mass-ratio (brighter star 20 times the fainter) is vitiated by the implicit assumption that Auwers' proper motion may be taken as the true proper motion of the centre of gravity of the system. I believe that it is the apparent resultant motion of the brighter star, and consequently it is only to be expected that its application should bring this star to relative rest. The inference that the brighter star is far the more massive I consider unsound. I had reached this conclusion before I saw the discussion of this system from spectroscopic observations by Dr Heber D. Curtis in *Lick Observatory Bulletin* No. 98. On the assumption that the two minor systems (each star being a spectroscopic binary) are coplanar with the major system, he reaches the conclusion that the mass of a_1 (the fainter component) is 6 times that of a_2. The assumption is of course tentative, but the resulting mass-ratio is probably more reliable than any that can be at present obtained from the meridian observations. I decided to adopt it as a trial hypothesis, and to see how it would work. Using the Greenwich observations collected on p. 215 of the Struve Memoir, the position of the centre of gravity reduced to 1890 with Auwers' proper motions and corrections, comes out as follows:—

Epoch of Catalogue.	Position of Centre of Gravity on the above Assumption of Masses.	
	R. A.	N.P.D.
	h m s	° ′ ″
1840	7 27 ...	57 52 16·99
1845	34·459	16·51
1850	34·544	16·99
1860	34·520	17·35
1864	34·491	16·86
1872	34·490	17·90
1880	34·547	17·91
1890	34·601	18·87
1900	34·566	18·57

From these I deduce a correction to the assumed proper motion of

	s	″
	+·0016	+·041
Auwers' value is	−·0151	+·079
Newcomb's being	−·0144	+·082

Hence the concluded proper motion of the centre of gravity of the system is $-^{s}\cdot0135$, $+''\cdot120$.

If the above mass-ratio is correct, we should expect a much more definite curvature in the position of a_2 than of a_1. It is

fortunate that this is the case, as observations of α_2 are much more numerous. I have accordingly reduced the following observations of α_2 to 1890, with the proper motion $-^s\cdot0135$, $+''\cdot120$.

Positions of α_2 reduced to 1890.

		h	m	s	°	′	″		
Bradley .	1757	7	27	34·88	57	52	21·64	} Adopt	$34^s\cdot83$ $21''\cdot6$
T. Mayer .	1757			34·74			21·54		
Piazzi .	1800			34·84			17·8		
Greenwich	1840			34·99			17·78		
,,	1845			34·94			17·24		
,,	1850			34·93			16·86		
,,	1860			34·93			16·43		
,,	1864			34·90			15·86		
,,	1872			34·88			15·73		
,,	1880			34·91			15·60		
,,	1890			34·91			15·45		
,,	1900			34·82			14·93		

These have been plotted on the diagram; it will be seen at once that, omitting Piazzi, which is discordant, they do indicate a curvature of about the required amount, as compared with Mr Lewis' diagram of the orbit. Hence they support the adopted mass-ratio as at least approximately correct. Obviously no curvature would be shown if α_2 had much the greater mass.

As Castor is a Greenwich clock star, it would seem to be advisable to follow the method that has already been adopted for Procyon, viz. to apply a double correction, one for the proper motion of the centre of gravity, the other for orbital motion round this point.

For example, in reducing the 1890 place of Castor to 1920 we should have to apply $-^s\cdot0135\times30-^s\cdot11$ to the R.A., and $+''\cdot120\times30-''\cdot56$ to the N.P.D. (in addition to precession). The second term is the orbital correction deduced from Mr Lewis' diagram on the assumption of mass-ratio 1 to 6.

These amount to $-^s\cdot405-^s\cdot11 = -^s\cdot515$
$+3''\cdot60-''\cdot56 = +3''\cdot04$

whereas if Auwers' proper motion were used they would be

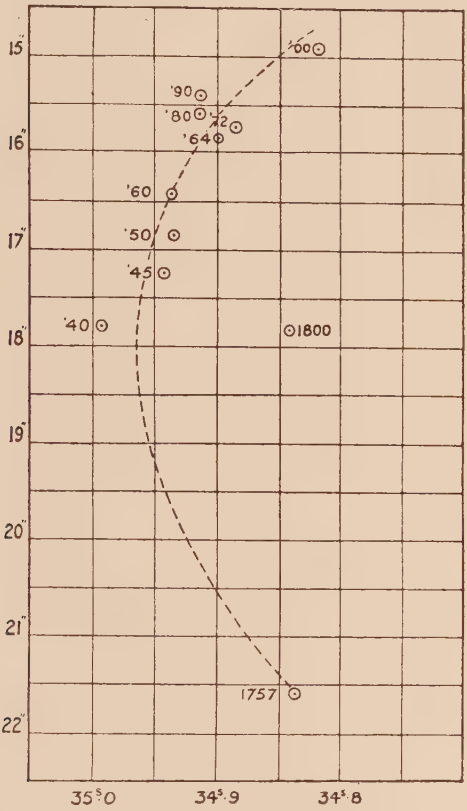
$$-^s\cdot0151\times30,\ +''\cdot079\times30 \quad = -^s\cdot453,\ +2''\cdot37$$

Hence even now the effect of the correction is sensible, and it will rapidly increase as periastron is approached. The direction of orbital motion in N.P.D. will be reversed in a few years, after which Auwers' and Newcomb's values of the motion in N.P.D. will become altogether erroneous.

This is, I believe, the first occasion when spectroscopic obser-

vations have assisted in determining the proper motion of a star. The possibility of mutual assistance of this kind forms another link connecting physical astronomy with astronomy of position.

Perhaps it is as well to call attention to an erratum in Mr Lewis' Memoir which misled me for a time and may mislead



Meridian Observations of α_2 Geminorum (Castor) reduced to 1890, with Proper Motion $-^{s}\cdot0135$, $+''\cdot120$.

others. On p. 21 Mr Furner's determination is printed as though the fainter star had 20 times the mass of the brighter. His determination on p. 215 of the Memoir made the bright star 20 times the faint, and, although the reverse is probably true, the two pages should be made consistent.

Estimate of the Number of Stars within Certain Limits of Proper Motion. By W. G. Thackeray.

The following data are derived from discussions of statistics of proper motions, published (1) by Prof. Auwers in the introduction of the Berlin "A" Catalogue of the Astronomische Gesellschaft series, pp. 141–143, where he discusses the Bradley proper motion referred to here as "Bradley," as well as certain of his zone stars referred to here as "Auwers," and though the separation of the stars is not in all cases identical with the grouping adopted in this paper, it is quite near enough for the purposes of this paper; (2) by Prof. Dyson and myself in the introduction to the New Reduction of Groombridge's Catalogue, p. xcii; and (3) by myself on some Carrington proper motions, published in the present volume of the *Monthly Notices*. The Bradley observations extend over the northern sky, Groombridge's lie within 52° of the pole, Carrington's within only 9°, and the Berlin "A" series are within the zone of +15° to +20° of Declination. The material under discussion cannot be considered to extend beyond 9·4 magnitude stars, but from the results it would appear as though the adopted percentages might be used further without much risk of appreciable error.

The agreement between the different catalogues is especially good for the fainter stars, and suggests the absence of any serious systematic error.

Percentage of Stars within Certain Limits of Centennial Proper Motions.

Authority.	0″–5″	5″–10″	10″–20″	>20″	No. of Stars.
	Magnitude 1–4·9.				
Bradley . .	42	22	18	18	711
Groombridge .	45	21	20	14	234
	43	22	18	17	
	Magnitude 5·0–5·9.				
Bradley . .	49	26	16	9	951
Groombridge .	59	21	14	6	538
	55	23	15	7	
	Magnitude 6·0–6·9.				
Bradley . .	52	29	13	6	1367
Groombridge .	66½	21	8	4½	1149
	60	25	10	5	

Authority.	0″–5″	5″–10″	10″–20″	>20″	No. of Stars.
	Magnitude 7·0–7·9.				
Bradley . .	55	26	13	6	188
Groombridge .	74	18	5½	2½	1369
Auwers . . .	76	14	7	2	1420
Carrington . .	64	22	12	2	114
	75	17	6	2	
	Magnitude 8·0–8·4.				
Groombridge .	78½	17	2½	2	940
Auwers . . .	80	11	7	2	1595
Carrington . .	69	19	9	4	179
	79	14	5	2	
	Magnitude 8·5–9·0.				
Auwers . . .	77	18	4	1	3366
Carrington . .	79	13	7	1	442
	78	15	6	1	
	Magnitude 9·0–9·4.				
Auwers . . .	79	13	7	1	1022
Carrington . .	80	15	5	1	378
	79	14	6	1	

With reference to the estimate of the number of the stars for the adopted groups of magnitude, Seeliger gives for the counts in the B.D. for the northern hemisphere the following figures :—

Mag. 1 –6·5	4,120 stars.
6·6–7·0	3,887
7·1–7·5	6,054
7·6–8·0	11,168
8·1–8·5	22,898
8·6–9·0	52,852
9·1–9·5	213,973
Total	314,952

and on this (Argelander) scale there would be 630,000 in the whole sky.

To exclude the 9·5 stars from this estimate, I have counted the number of 9·5 stars on thirteen pages in different volumes of the B.D., which gave 98, 95, 107, 107, 100, 125, 81, 124, 143, 131, 93, 75, and 105 respectively, or an average of 106·5 out of every 300

stars, say 35 per cent. Excluding these, the number of stars down to 9·4 magnitude would thus be some 400,000.

The adopted values of the numbers for the different groups of magnitudes, and the percentage values within certain limits of proper motion, are given in the following table :—

Percentage of Stars within Certain Limits of Centennial Proper Motions in Order of Magnitude, with Estimated Number of Stars.

Mag.	0″–5″	5″–10″	10″–20″	>20″	Estimated No. of Stars in Thousands.
1 –4·9	43	22	18	17	1
5·0–5·9	55	23	15	7	3
6·0–6·9	60	25	10	5	12
7·0–7·9	75	17	6	2	35
8·0–9·4	79	14	6	1	350

Estimated Number of Stars within Certain Limits of Centennial Proper Motions in Order of Magnitude.

1 –4·9	430	220	180	170	1
5·0–5·9	1,650	690	450	210	3
6·0–6·9	7,200	3,000	1,200	600	12
7·0–7·9	26,250	5,950	2,100	700	35
8·0–9·4	276,500	49,000	21,000	3,500	350
	312,030	58,860	24,930	5,180	401

From a comparison of the Groombridge and Carrington proper motions, for the purpose of estimating the effect of accidental error, the probable error of a centennial proper motion in N.P.D. is ±0″·8 and in R.A. ±0″·9 ; thus the probable error of a resultant centennial proper motion is ±1″·2. It would, therefore, be reasonable to infer that the numbers corresponding to these limits of proper motion would not be liable to any serious alteration for the effect of accidental error of observation.

If the group 0″ – 5″ is further broken up it will be found that for all magnitudes the stars tend to accumulate somewhere round 2″·5 as a resultant centennial proper motion, and this seems too large a quantity to be due to systematic or accidental error.

Notes on some Proper Motions derived from a Comparison of Carrington's Catalogue with the Greenwich Places for 1900. By W. G. Thackeray.

In the Greenwich Catalogue for 1900, which is now in course of construction, there are 1185 stars to be found in Carrington's Circumpolar Catalogue for 1855. Of these stars, 94 common to the Groombridge-Greenwich system have been used to discuss the

systematic corrections applicable to Carrington's places to bring them into line with the Groombridge-Greenwich system, and the results have been published in *Monthly Notices*, lxvi. pp. 320–323.

After applying these corrections to the Carrington Catalogue places, proper motions for these 1187 stars have been derived by a simple comparison of the Greenwich observed places for 1900 with the corrected Carrington places brought up to 1900 by Struve-Peters precession constants.

Taking those stars which are common to Groombridge and Carrington, and forming the differences of proper motion derived from the Groombridge and Carrington Catalogue places respectively in the sense of correction to Groombridge for every three hours of right ascension, we get corrections which can be compared with the tables of corrections given by Boss in his paper, "The New Reduction of the Meridian Observations of Groombridge" (*Monthly Notices*, lxvi. p. 513).

The tables are given in the form of corrections to proper motion in arc in both elements. Boss's corrections in N.P.D. are not given, as being practically insensible.

Boss's corrections in R.A. are found by multiplying by $\frac{3}{16}$ the quantities given in Table II., Zone IV., *Monthly Notices*, lxvi. p. 563.

Corrections to Groombridge's Proper Motions in Order of R.A.

R.A.	Right Ascension.		N.P.D.
	Boss.	Carrrington-Greenwich.	Carrington-Greenwich.
	″	″	″
0h	·000		
1½		+·003	−·003
2	+·009		
4	−·006		
4½		−·003	−·010
6	−·010		
7½		−·010	−·005
8	−·016		
10	−·001		
10½		+·003	−·005
12	+·017		
13½		+·020	−·003
14	+·016		
16	+·014		
16½		+·013	−·002
18	·000		
19½		−·004	−·004
20	+·002		
22	−·006		
22½		+·002	−·001

The agreement between Boss and Carrington is notable, and shows the excellence of Carrington's observations. The small

mean difference in N.P.D. represents but a small systematic discordance.

Again arranging these Carrington proper motions in order of magnitudes and in octants of R.A., in the same manner as was adopted in the case of the Groombridge Catalogue, we get the following tables of numbers and percentages:—

Numbers of Stars—Centennial Proper Motions in Order of Stars' Magnitude.

Magnitude.	Total Number of Stars.	0″–5″	5″–10″	10″–20″	>20″
m m					
5·0–5·9	19	5	8	6	0
6·0–6·9	46	28	12	4	2
7·0–7·9	114	73	25	14	2
8·0–8·4	179	121	35	16	7
8·5–8·9	442	338	82	17	5
9·0–	378	302	56	17	3
Total .	1178	867	218	74	19

Percentage of Stars—Centennial Proper Motions in Order of Stars' Magnitude.

m m					
5·0–5·9	19	26	42	32	0
6·0–6·9	46	61	26	9	4
7·0–7·9	114	64	22	12	2
8·0–8·4	179	69	19	9	4
8·5–8·9	442	77	18	4	1
9·0–	378	80	15	5	1

Percentage of Stars—Centennial Proper Motions in Order of R.A.

Limits of R.A.	Total Number of Stars.	0″–5″	5″–10″	10″–20″	>20″
h h					
0– 3	149	67	28	5	1
3– 6	127	77	16	7	0
6– 9	137	80	12	7	2
9–12	170	70	22	5	3
12–15	172	71	20	8	1
15–18	140	75	19	4	2
18–21	147	82	15	2	1
21– 0	136	74	11	13	2

The mean discordance of a determination of proper motion from Groombridge and Carrington is ±″·010 in N.P.D. and ±″·011 in R.A., using those stars only which have little or no systematic error which would give a probable error for a resultant centennial proper motion of 1 ±″·2 as the effect of accidental error.

On the Accidental Production of Temporary Errors of Division on a Graduated Circle. By W. M. Witchell.

(*Communicated by the Astronomer Royal.*)

During an examination, undertaken recently, of the micrometer screws of the Greenwich Meridian Circle reading microscopes, a suggestive discovery was made.

Six observations of "runs" had been taken over each of three consecutive intervals of 5′, and these agreed among themselves quite normally so long as the same interval was under consideration; but when the results from the different intervals were compared a discordance much beyond the probable accidental error of observation appeared between the values obtained at pointer reading 89° 35′–40′, and those at 89° 40′–45′.

As will be seen from the following figures, the discordance amounted to 0ʳ·014 or 0″·84, and an attempt to trace its origin was necessary.

Value of 5′ of Circle in "Mean Micrometer."

	Pointer 89° 35′ to 89° 40′.	Pointer 89° 40′ to 89° 45′.
	r	r
Set 1 . .	4·903	4·893
,, 2 . .	·912	·892
,, 3 . .	·908	·897
,, 4 . .	·907	·894
,, 5 . .	·904	·893
,, 6 . .	·908	·891
	4·907	4·893

Difference : 0ʳ·014

An accidentally large deviation from the mean division error at this part of the circle was at first suspected. A search for others of like magnitude in the neighbourhood, however, produced negative results. But when these observations (which consisted of three sets of runs over each 5′ interval from pointer reading 88° 20′ to 89° 20′) were arranged so as to exhibit the values from the six micrometers individually, and were compared with the former series similarly arranged, it was at once seen that the discordance took its origin in a large apparent error of the particular graduation viewed by microscope D when the pointer reads 89° 40′. The figures follow. They were considered to give strong evidence of an error amounting to 0ʳ·03 of micrometer, or 1″·8 in this graduation, inasmuch as the mean screw measurements of the two adjacent intervals appeared to be too large and too small respectively by approximately this quantity.

Measurement of 5′ *Interval*, 89° 35′–89° 40′ *pointer reading.*

Micrometer	A	B	C	D	E	F
	r	r	r	r	r	r
Set 1 . .	4·910	4·905	4·887	4·931	4·909	4·874
,, 2 . .	·914	·917	·915	·937	·914	·876
,, 3 . .	·927	·908	·899	·927	·918	·870
,, 4 . .	·911	·920	·897	·923	·910	·883
,, 5 . .	·899	·918	·896	·912	·906	·891
,, 6 . .	·920	·921	·891	·924	·901	·889
Mean .	4·914	4·915	4·898	4·926	4·910	4·880

Corresponding means of 36 other sets, viz. 3 over each 5′ interval from 88° 20′ to 89° 20′:—

4r·923	4r·918	4r·890	4r·901	4r·916	4r·884

Measurement of 5′ *Interval*, 89° 40′–89° 45′ *pointer reading.*

Micrometer.	A	B	C	D	E	F
	r	r	r	r	r	r
Set 1 . .	4·916	4·921	4·871	4·868	4·902	4·880
,, 2 . .	·913	·910	·869	·867	·905	·885
,, 3 . .	·917	·892	·881	·873	·916	·902
,, 4 . .	·910	·899	·889	·890	·902	·876
,, 5 . .	·896	·916	·868	·875	·911	·890
,, 6 . .	·925	·908	·891	·854	·905	·863
Mean .	4·913	4·908	4·878	4·871	4·907	4·883

Now in the winter months it has been customary to protect the circle from tarnish by applying to it a thin film of vaseline, which, however, gradually accumulates small particles of dust and is removed from time to time.

Before proceeding to a systematic examination of the circle for possible errors of like nature in other graduations, the part under actual suspicion was wiped clean; when it was found that the supposed error of nearly 2″ in the division in question had absolutely disappeared, presumably with the dusty vaseline.

It should be stated here that the microscopes were in good focus and adjustment.

Other divisions being similarly experimented upon gave no difference, before and after, at all comparable with the foregoing, except in one case (the figures for which are quoted below), so that the liability to error introduced by this method of preserving the circle is probably both slight and casual. At the same time it is distinctly real, and may be the explanation of discordances which cannot be ascribed with certainty to other agencies.

The following are micrometer readings for ten bisections of the division under microscope E (pointer reading 164° 45′) before and after wiping off the vaseline:—

Before.		After.	
	r	r	r
608	·611	·633	·618
·613	·612	·627	·620
·603	·604	·629	·624
·589	·593	·620	·632
·600	·590	·622	·624
Mean r·602		Mean r·625	

Difference : r·023 = 1″·40

As a consequence of these revelations, the cleaning of the circle, which hitherto has naturally been avoided as much as possible for fear of injuring the graduations, will be carried out more frequently.

Observations of Minor Planets from Photographs taken with the 30-inch Reflector of the Thompson Equatorial at the Royal Observatory, Greenwich, during the year 1903.

(*Communicated by the Astronomer Royal.*)

The following positions of minor planets were obtained from photographs taken with the 30-inch Reflector during the year 1903.

The plates were measured with the astrographic micrometer. Four reference stars were, as a rule, measured with the planet, their positions being derived when possible from the Catalogues of the Astronomische Gesellschaft.

The positions given are not corrected for Parallax.

log Parallax Correction = log Parallax Factor − log Δ.

The anonymous planet was found on the same plate as (407) Arachne.

Date and G.M.T. 1903.		Apparent R.A.	Apparent Dec.	Log. Parallax Factor.	
				R.A.	Dec.
d	h m s	h m s	° ′ ″		
		(258) Tyche.			
May 25	10 41 29	14 38 6·48	−7 40 58·1	+8·321	+0·876
26	9 55 41	14 37 25·65	−7 34 56·3	+8·669	+0·876
		(68) Leto.			
May 29	11 25 13	15 13 48·14	−20 43 7·1	+8·723	+0·921
June 2	10 46 9	15 10 18·77	−20 38 24·5	+8·462	+0·921
3	10 29 3	15 9 28·86	−20 37 15·3	+7·931	+0·922
4	10 4 18	15 8 40·18	−20 36 7·5	−8·408	+0·921

Date and G.M.T. 1903.				Apparent R.A.			Apparent Dec.			Log. Parallax Factor. R.A.	Dec.
d	h	m	s	h	m	s	°	′	″		
					(17) Thetis.						
May 29	11	58	12	16	59	51·84	−14	11	6·9	−8·772	+0·902
June 2	11	18	49	16	56	12·69	−14	11	50·4	−8·961	+0·900
3	11	11	36	16	55	16·89	−14	12	15·7	−8·983	+0·900
4	11	47	10	16	54	18·99	−14	12	45·5	−8·472	+0·902
22	11	5	57	16	38	20·83	−14	38	54·8	+8·664	+0·904
					(511) Davida.						
June 2	11	48	24	16	49	57·68	−9	13	4·5	−8·511	+0·883
3	12	9	25	16	49	10·12	−9	13	46·2	+7·909	+0·883
4	11	27	1	16	48	24·63	−9	14	30·2	−8·726	+0·883
					(304) Olga.						
June 3	11	42	3	15	29	10·14	+8	17	20·4	+8·960	+0·779
4	10	28	42	15	28	23·81	+8	17	39·8	−8·249	+0·778
					(434) Hungaria.						
June 2	12	23	56	16	16	25·39	+22	29	49·8	+8·925	+0·631
4	10	57	43	16	14	36·50	+22	36	36·4	−8·684	+0·626
22	10	35	59	16	0	57·07	+21	55	8·2	+8·779	+0·636
24	10	22	53	15	59	57·33	+21	40	3·6	+8·711	+0·640
					(432) Pythia.						
June 22	11	27	42	16	52	3·91	−19	46	40·1	+8·760	+0·919
27	10	36	45	16	47	36·66	−20	26	1·9	+8·077	+0·921
July 1	10	36	53	16	44	29·33	−20	57	31·9	+8·647	+0·922
					(405) Thia.						
June 22	11	54	50	17	23	0·93	−19	29	50·0	+8·733	+0·918
27	11	43	24	17	18	40·46	−18	53	17·8	+8·878	+0·915
July 1	11	5	2	17	15	36·32	−18	26	19·0	+8·624	+0·915
					(270) Anahita.						
Aug. 4	11	57	52	20	22	25·63	−15	13	57·6	+8·616	+0·906
5	10	38	24	20	21	32·44	−15	16	5·6	−8·915	+0·904
10	10	29	57	20	17	3·00	−15	27	24·4	−8·749	+0·906
13	9	58	44	20	14	35·56	−15	34	7·1	−8·924	+0·905

Date and G.M.T. 1903.	Apparent R.A.	Apparent Dec.	Log. Parallax Factor. R.A.	Dec.
	(57) Mnemosyne.			
d h m s	h m s	° ′ ″		
Aug. 4 11 39 53	19 39 27·24	+0 10 56·1	+8·899	+0·835
5 9 44 42	19 38 50·55	+0 6 49·2	−8·984	+0·835
6 10 9 25	19 38 10·50	+0 2 8·5	−8·694	+0·836
7 10 6 44	19 37 31·86	−0 2 33·6	−8·665	+0·836
10 10 9 34	19 35 40·30	−0 17 17·0	−8·317	+0·838
13 9 28 56	19 33 56·84	−0 32 43·2	−8·803	+0·839
	(147) Protogenia.			
Aug. 5 11 8 19	21 24 0·62	−12 32 43·0	−9·123	+0·892
13 11 7 46	21 17 52·19	−12 59 26·6	−8·865	+0·897
22 11 14 1	21 11 5·03	−13 29 53·9	+7·693	+0·900
	(407) Arachne.			
Aug. 5 11 25 49	21 44 50·01	−7 53 4·4	−9·127	+0·875
6 10 59 25	21 43 58·25	−7 53 50·5	−9·221	+0·873
31 11 25 11	21 21 21·29	−8 33 47·6	+8·808	+0·880
Sept. 1 9 59 38	21 20 36·37	−8 35 37·0	−8·819	+0·880
	(Anonymous.)			
Aug. 6 10 59 25	21 40 47·82	−7 20 8·6	−9·209	+0·871
31 11 25 11	21 20 30·19	−8 17 4·9	+8·819	+0·879
Sept. 1 9 59 38	21 19 52·08	−8 19 35·8	−8·819	+0·879
	(324) Bamberga.			
Aug. 31 11 51 34	22 3 40·60	−9 43 8·3	+8·589	+0·885
Sept. 1 10 17 35	22 2 42·34	−9 37 21·3	−9·023	+0·883
7 9 40 11	21 56 45·23	−8 59 41·2	−9·071	+0·880
9 10 12 59	21 54 53·28	−8 46 38·3	−8·699	+0·881
11 10 32 9	21 53 7·53	−8 33 31·1	−7·509	+0·881
	(333) Badenia.			
Sept. 11 10 58 46	22 32 49·07	−10 51 22·3	−8·387	+0·890
	(184) Dejopeja.			
Aug. 31 12 54 27	22 41 3·51	−8 13 1·4	+8·895	+0·878
	(514) 1903. M. B.			
Aug. 31 13 20 47	22 54 43·86	−0 54 48·9	+8·991	+0·841
	(513) 1903. L. Y.			
Aug. 31 12 20 27	23 11 34·82	+0 28 50·7	−8·379	+0·833

Enhanced Lines of Iron in the Region F to C. By A. Fowler.

In view of the now generally recognised importance of enhanced lines in the interpretation of solar and stellar spectra, it is thought that the accompanying observations of the enhanced lines of iron which occur in the less refrangible parts of the spectrum may be useful to other workers.

The lines have been observed and photographed under various conditions: in the spark, in the arc at reduced pressure, in the arc in hydrogen, and on the positive pole of an ordinary continuous-current arc in air at atmospheric pressure, metallic electrodes being used throughout. There is a distinct gain in producing the lines without the use of a spark in a few cases, inasmuch as there is no air spectrum to interfere with their detection.

The occurrence of the enhanced lines on the positive pole of the arc affords a particularly convenient mode of identifying them, except towards the red, where the continuous spectrum tends to mask the fainter lines. When the bright spot on the positive pole is carefully adjusted on the slit, the lines in question are observed as very short lines, quite different in appearance from the arc lines, which are also present and provide a convenient reference spectrum; unlike the enhanced lines, the arc lines are either weakened or unchanged on the positive pole. A similar appearance is observed on the negative pole, but the enhanced lines are not so bright. In these experiments the current has ranged from 12 to 0·4 ampères, on 110-volt circuit, with an approximately constant potential difference of 40 volts between the electrodes, and the intensities of the enhanced lines have not been found to be materially changed as compared with the arc lines observed at the same time. Even with 0·4 ampères, the arc burning continuously, all the arc lines remained visible when the proper part of the image was brought on the slit, and the enhanced lines were still very distinct in the immediate neighbourhood of the poles.

This result is somewhat different from that obtained by Hartmann with magnesium poles, in which case the enhanced line 4481 is greatly strengthened as the current is reduced. It differs also from Prof. Hale's recent observations of the iron spectrum,* in which a 2-ampère arc was found to give a spectrum closely approximating to that of the outer flame of an ordinary arc with greater current, though no material change was observed in passing from 30 to 15 ampères. It may be that the difference is partly due to the use of metallic electrodes in my experiments, while Prof. Hale appears to have used the metal on carbon poles; under the latter conditions I have obtained similar results with the 2-ampère arc, but only when the quantity of iron on the poles was small. At all events, when metallic poles are used, reduction of current strength does not appear to be accompanied by a reduction of temperature sufficient to produce any notable differences in the

* *Astrophys. Journal*, vol. xxiv. p. 208.

spectrum, if corresponding parts of the arc be observed each case. Indeed, this observation accords well with Prof. J. J. Thomson's remark * that in the arc "the temperature of the crater of the positive terminal remains constant even when the current varies."

It should be remarked, however, that the similarity of the phenomena in the 2-ampère iron arc with those observed when the current strength is greater does not in the least invalidate Prof. Hale's conclusion as to the probable low temperature of sun-spots. As in my own discussion of this point,† Prof. Hale's result ultimately depends upon a comparison of spot spectra with the spectrum of the arc-flame.

The wave-lengths of all the lines given in the table have been determined from a new series of photographs in which the linear dispersion from C to F is 24 cm. The results have differed so slightly (rarely more than 0·02) from solar lines of appropriate intensity tabulated by Rowland that there can be no doubt as to their identity, and to avoid any possible confusion, Rowland's wave-lengths have been adopted. This procedure is, in fact, justified by Lockyer and Baxandall's demonstration ‡ of the presence of enhanced lines of iron in the more refrangible parts of the solar spectrum.

The representation of the additional lines with proper intensities in the Fraunhofer spectrum, together with their special behaviour in the chromosphere and spots, § is valuable confirmation of their classification as enhanced lines. A few lines, notably two at wave-lengths 5260·50 and 5100·95 which appear in some of the spectra, have not been included in the table because they failed to satisfy these conditions, although the probable impurity producing them has not yet been traced. That such lines were due to some substance other than iron was further suggested by their variable intensities with respect to undoubted enhanced lines. Prof. Hale has met with a somewhat similar case in a supposed enhanced line of iron at 5218·37, of which he says that "the enhancement of this line may vary." Here, however, there can be no hesitation in attributing the line to copper, which is a very common impurity in iron.

There is, in fact, no reason to suppose that the intensities of the enhanced lines are appreciably different relatively to each other under any of the conditions of experiment which have been mentioned, though their intensities, as compared with the arc lines, are not the same in all cases. The enhancement is most marked in the spark spectrum. Estimates of the relative intensities are given in the table.

For the sake of completeness, the table also shows the behaviour of the lines in the chromosphere and sunspots, Y, H, M, and F respectively indicating Young, Hale, Mitchell, and Fowler.

* *Discharge of Electricity through Gases* (1903), p. 417.
† *Trans. Int. Solar Union*, vol. i. p. 228 (1906).
‡ *Roy. Soc. Proc.*, vol. lxxiv. p. 225 (1904).
§ *Monthly Notices*, vol. lxvi. p. 361 (1906).

Enhanced Fe. Wave-length.	Int.	⊙ Rowland. Origin.	Int.	Behaviour in Sun-Spots.	Intensity in Chromosphere. Y	M	F	Remarks.
4924·11*	9	Fe	5	Weakened H, F	10	15	25	
5018·63*	9	Fe	4	,, H, F	15	10	25	
5169·22*	10	Fe	4	,, H, M, F	25	25	40	
5197·74†	4	...	2	,, H, M, F	10	15	25	
5234·79†	4	...	2	,, H, M, F	10	15	25	
5264·98	1	...	0	,, H, F	3	7	15	
5276·17*	6	Fe?	3	Not clearly affected M, F	10	8	35	Compound line in ⊙, 76·24, 76·17.
5316·79*	9	Fe	4	Weakened Y, H, F	2–20	20	45	
5325·74	2	...	(2)	,, Y, F	2	10	15	Rowland's ⊙ int. too high.
5363·06†	5	...	3	,, F	5–10	8	30	
5535·06†	4	Fe	2	,, H, M, F	12	12	35	Masked by air line in spark.
6238·50	3	...	2	,, M, F	2	...	15	
6247·77	3	...	2	,, M, F	4	...	20	Observed also in spark by Mitchell.
6417·13	2	Fe?	1	,, M, F	2	...	...	
6456·60†	6	...	3	,, M, F	3–10	5	20	

It will be seen that the representation of the enhanced lines in the solar spectrum, and in the spectrum of the chromosphere and sun-spots, is quite consistent throughout. As in the case of Lockyer and Baxandall's investigation of the more refrangible parts of the spectrum, origins are now provided for several lines unidentified or doubtfully identified by Rowland, the reason being that he did not ordinarily obtain the weaker enhanced lines in his photographs, and probably attributed their occasional appearance to impurities.

There is abundant evidence that all the enhanced lines of iron in the region F to C are weakened in the spectra of sun-spots, and the work at Mount Wilson has already shown that the same is true of many of the lines of this class in the blue and violet.

All the lines in question are also prominent in the spectrum of the chromosphere, and, according to my previous observations, are of the high-level type, with the possible exception of 6417·7, which is not yet included in my list, but is given by Young.

It is sufficiently clear, therefore, that the enhanced lines constitute a special system of lines which vary together both in laboratory experiments and in the various parts of the sun where they are observed.

Note.—It may be mentioned that, while generally confirming Lockyer's list of enhanced lines of iron in the more refrangible parts of the spectrum, the special photographs of the arc reveal an additional line of considerable intensity at 4416·98, which is masked by an air line in the spark. This line also is weakened in the sun-spot spectrum.

* Previously recorded by Lockyer, *Pub. Sol. Phys. Obs.*, 1906, etc.
† ,, ,, Author, *Monthly Notices*, vol. lxvi. p. 364, 1906.

Note on Silicon in the Chromosphere. By A. Fowler.

Pending a more complete investigation of the spectrum of silicon in relation to the chromosphere and Sun-spots, it may be of interest to draw attention to the identification of two strong red lines of this element with well-marked chromospheric lines. The lines in question have been previously observed in the spark spectrum by Salet (λλ 6341, 6366), by the Count de Gramont (λλ 6342, 6370), and more recently by Lunt, who gives the approximate wave-lengths 6346·9 and 6371·2.*

A careful re-determination of the positions of these lines, from photographs giving a linear dispersion of 10 tenth-metres to the millimetre in this part of the spectrum, leaves no doubt as to their coincidence with the previously unidentified high-level chromospheric lines at 6347·31 and 6371·57. The more refrangible of the two lines is the stronger, in the proportion of about 10 to 6, and their intensities in the chromosphere, according to my own observations,† are 25 and 15 respectively. Both lines occur in the Fraunhofer spectrum, with intensities and characters 2N and 1Nd? respectively, and Rowland assigns the latter to iron, while leaving the other unidentified. Kayser and Runge also give a faint line at 6371·60 in the arc spectrum of iron, but it does not appear on my photographs of the iron spectrum, except when the presence of silicon is indicated by the other line at 6347·31. In any case, if there be an iron line at 6371·57, it is not an enhanced line, and is not of sufficient intensity in the arc to account entirely either for the Fraunhofer or chromospheric line at the same wave-length, which must accordingly be attributed chiefly to silicon.

In Sun-spots, according to the observations of Mitchell‡ and myself, the two lines are almost obliterated, so that there is a complete agreement of behaviour and intensities throughout.

The relationships of the different families of silicon lines have not yet been fully worked out, but it is probable that the two red lines, like so many of the other high-level chromospheric lines which are weakened in spots, belong to the enhanced line class. Like the enhanced lines of iron, they appear close to the positive pole when a little silica is introduced into the iron arc, and the wave-lengths have been determined by reference to adjacent iron lines which occur with the silicon lines under these conditions. In his recent discussion of the violet part of the flash spectrum Prof. Dyson§ concluded that there was "a fair degree of probability for the existence of silicon in the chromosphere," and all doubt is now removed by the identification of the red lines.

* *Annals of the Cape Observatory*, vol. x., Part II., p. 18B (1906).
† *Monthly Notices*, vol. lxvi. p. 365 (1906).
‡ *Astrophys. Jour.*, vol. xxiv. p. 92 (1906).
§ *Phil. Trans.*, vol. ccvi. A, p. 440 (1906).

Notes on some Spectroscopic Observations of the Sun.

By H. F. Newall, M.A., F.R.S.

Spectroscopic observations of the Sun have been carried on during the past year at Cambridge. In the earlier part of the year the observations were made with the 25-inch equatorial (the Newall telescope), to which a diffraction-grating spectroscope was attached. In the later months a fixed horizontal telescope was improvised, light being directed into it by means of a cœlostat and an auxiliary mirror; and a much more powerful grating spectroscope was used. It will be convenient to refer to these two equipments as the equatorial and the horizontal.

The general programme was to test by preliminary observations, whether the conditions of atmosphere over the flat plains of Cambridgeshire would justify a special outfit which should enable us to take some active part in solar work. It seemed that the study of sun-spot spectra would be a fitting one for making the trials. For, in this work, not only is a fairly clear atmosphere desirable, such as will not prevent the detection of faint characteristic detail in the sun-spot spectra by the overpowering effect of integrated sunlight scattered by intervening haze; but also a steady atmosphere is needed, such as will not give rise to jumping images and the blurring effects due to the spread of circumpenumbral light over the umbra in consequence of atmospheric tremor.

Accordingly a programme was adopted to include spectroscopic observations of sun-spots and other details on the solar surface, but not observations of prominences at the limb.

My idea was that it would be best, in the first instance, to utilise the equatorial equipment to gain practical ideas of the difficulties to be met, and the advantages to be gained, in direct pointing at the Sun with a telescope whose object-glass would in general be about 30 feet above the ground. And then, if the observations proved to be generally encouraging as regards atmospheric conditions, a horizontal equipment was to be tried, with the beam about 6 feet above the ground.

The result of these trials has been completely satisfactory and decisive. The observations with the equatorial equipment showed that good conditions of observing were available in the early morning between 6 and 9 A.M.

The observations with the horizontal equipment showed that these conditions are not seriously impaired by bringing the beam nearer to the ground; any deterioration arising from greater proximity to the ground is probably more than balanced by the vastly freer circulation of air in the path of the optical beam in the open dome as opposed to the closed tube of the equatorial. The conveniences of working with the horizontal equipment—on a fixed floor, with fixed apparatus, with eye-piece in constant and convenient position, with all the parts easily accessible, and most of

all, with the power to use very powerful spectroscopes—can hardly be overstated.

The Equatorial Equipment.—For these solar observations the aperture of the 25-inch object-glass was reduced to 12 inches, and the object-glass was usually between 25 and 30 feet above the ground.

A powerful grating spectroscope was attached to the equatorial in the autumn of 1905. A special mounting of a plane grating had been constructed to replace the prism-head of the four-prism spectrograph which has been used in stellar work (*M.N.*, lxv., Plates 18, 19). The beam which issues from the collimator is

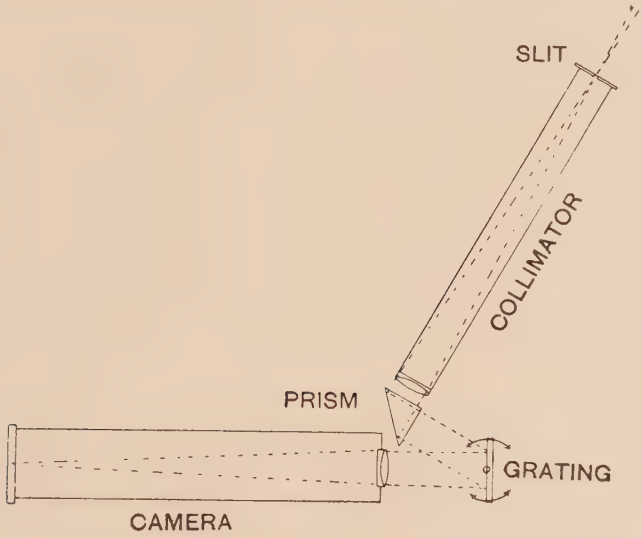


Fig. 1.

deflected through a right angle by a prism fixed in the grating-head; the deflected beam falls on the grating, and the camera is fixed so as to receive a beam emergent at an angle of 30° to the incident beam, and at an angle of 120° to the axis of the equatorial and true collimator. Different orders of spectrum are brought into the field of view by turning the grating, which is geometrically mounted in a cast-iron frame capable of rotation about an axis coincident with the middle ruling of the grating.

The grating is one ruled by Rowland; the ruled surface has an area $3\frac{1}{2}$ inches × 2 inches on speculum metal, and the lines are ruled 14,438 to the inch.

The grating and collimator and camera are of such dimensions as to transmit a 2-inch beam. The ratio a/f is $\frac{1}{10}$ for both collimator and camera, the effective aperture of the diaphragmed object-glass of the equatorial is only $\frac{1}{21}$, but the slit of the spectro-

scope is adjusted to such narrowness that the diffractional spread of the beam in the collimator fills the lens laterally, and consequently the resolving power of the grating in a 2-inch beam is utilisable.

The spectrum of the second order has generally been used; and of the two spectra of that order, that in which the dispersion and magnification are greater has been chosen. The grating has been adjusted in its frame in such a way that this special spectrum shall be the brightest available.

In the winter months work with the equatorial was sometimes interrupted by condensation of moisture on that surface of the object-glass which is nearest to the observer. Openings were cut through the tube of the equatorial close to the object-glass; and ventilation, controlled by shutters of the "hit-and-miss" type, was promoted. The troubles of condensation did not recur.

The metal diaphragm, used in the first instance to cut down the aperture of the object-glass to 12 inches, was found to cause great disturbance of the image on the slit. When this easily heated diaphragm was shielded by white cardboard and supplemented by a short tube (12 inches in diameter) made of tin-sheet and projecting about 12 inches outwards towards the Sun, the air currents were in part mitigated and in residue diverted from the path of the beam used in observation.

One of the 4-inch finders attached to the equatorial was provided with a projection screen, and was used in setting any desired spot into the field of view of the guiding eye-piece of the spectroscope.

It was found of great convenience to get a quick record of the spots visible on the disc, together with means of orientation. Photographic "printing-out" paper was laid on the projection screen, and half-a-minute's exposure to the Sun's image was enough to give a negative print of the requisite density. Orientation was given by the properly adjusted cross-wires in the focal plane of the finder; they appear as white lines on the negative print of the Sun's disc.

The Horizontal Equipment.—The object-glass of the 25-inch equatorial was dismounted, and fixed in a stout wooden frame, with its optical axis horizontal at a height of about 6 feet above the ground. The aperture was reduced to 12 inches, and sometimes to 6 inches. A 16-inch cœlostat was set up in a small building to the south of the dome of the Newall telescope. By an auxiliary mirror (also 16 inches) the beam from the cœlostat was reflected northwards through an opening made in the dome, and therein the beam passed through the object-glass, which was set with its axis pointing northwards along a line which lay a few feet to the west of the pillar of the temporarily disused equatorial. The object-glass was placed within a few feet of the south wall of the dome; and as the dome is 40 feet in diameter whilst the focal length of the object-glass is 29 feet, there was room enough to set up a large spectroscope in the northern part of the dome, with the slit in the focal plane of the object-glass.

The spectroscope used was the fine instrument built by Messrs Cooke of York, for the late Professor Piazzi Smyth; it has been put at the disposal of the Observatory by the Royal Society. The lenses of the collimator and camera are 4 inches in diameter. The focal length of collimator is 53 inches, and that of the simple camera is 67 inches; a negative enlarging lens can be inserted in the camera, and it has been used in such adjustment as to give an effective focal length of 12 feet 9 inches for the camera.

The plane grating used with this instrument is one ruled by Rowland, and has a ruled surface of 5 inches by $3\frac{1}{2}$ inches on speculum metal, and the lines are ruled 14,438 to the inch. The second order of spectrum was generally used. The slit was usually made narrow enough to give a diffractional indicator less than 1 on the 4-inch lens of the collimator (*M.N.*, lxv. p. 611). Records of the positions and number of spots were printed photographically as in the case of the equatorial equipment; but in this case the printing-out paper was held on the slit-plate in the focal plane of the object-glass, orientation being recorded by the shadow of a wire stretched in front of the photographic paper.

The horizontal equipment was put together in a temporary manner, but in building the cœlostat house and making the windows in the wall of the dome, all arrangements were made in such a way that, if the preliminary observations proved satisfactory, a permanent equipment could be installed, on the same general lines, but with this following modification :—

Permanent Horizontal Equipment in Process of Construction.

The cœlostat house is to contain the cœlostat and auxiliary mirror and an adjustable object-glass of aperture 12 inches and focal length about 60 feet. The focal plane of this horizontal telescope will lie in the North Annex, a convenient laboratory on the north side of the dome of the Newall telescope. The spectroscopic and other apparatus for studying the image of the Sun will be entirely in the annex. The dome itself will be used simply as an optical gallery so far as the solar equipment is concerned; the equatorial will be thus completely available for stellar work, unhampered by any solar apparatus in the dome.

For the solar work, the beam from the cœlostat to the annex will thus pass through shady places well protected from direct solar radiation and from disturbance from detrimental air currents.

The cost of the equipment for solar observations, which represents a new departure at the Cambridge Observatory, will be met by an appropriation from the Frank McClean bequest.

Meteorological Conditions between 1905 *Nov.* 24 *and* 1906 *Sept.* 30 (310 days).

The following table summarises the state of the sky for observing purposes. "Bright" means that for at least 2 hours in the day there was a cloudless sky.

		Overcast.	Bright.	Sunshine between Clouds.	Haze.	Observing Days.
1905	Nov. . .	5	2	...	...	1
	Dec. . .	22	7	2	...	6
1906	Jan. . .	14	6	10	1	8
	Feb. . .	18	2	6	2	3
	Mar. . .	22	7	2	...	4
	Apr. . .	13	9	7	1	2
	May . .	23	4	3	1	2
	June . .	14	5	8	3	4
	July . .	16	12	1	...	9
	Aug. . .	13	13	2	1	15
	Sept. . .	13	11	4	2	5
		173	78	45	11	59
				134		

No record on 3 days.

Nearly all observers agree that the early hours of the morning are the most favourable for solar observations. Experience soon showed that Cambridge was no exception to the general rule, and it is probably no exaggeration to say that two minutes' observations between 6.0 and 9.0 A.M. reveal more than an hour's work near midday. Nearly all the observations after May were made in the early hours.

Observations on the Nature and Amount of Sky-glare.

Sun-spots are observed under trying conditions. We look at them through a bright sunlit veil in the Earth's atmosphere. If the veil is illumined by the Sun to such an extent that the veil is as bright as the umbra, then it is to be expected that details in the spectrum of the umbra will only be discerned with difficulty.

Evershed's experiments * led him to the view that in the green and yellow part of the spectrum the brightness of the umbra was about $\frac{1}{20}$th of that of the Sun's surface. W. E. Wilson found † that his measures with Boys' radiomicrometer indicated a ratio of about $\frac{1}{5}$.

If we adopt Evershed's value as applicable to observations in the visual part of the spectrum, we should expect that characteristic details in the sun-spot spectrum would be difficult to discern, if the brightness of the sky-glare were $\frac{1}{20}$th of that of the solar surface.

Now this veil or sky-glare is to be attributed to two main causes: (1) scattering by fog or haze or air, (2) scattering by atmospheric tremor. The first of these, acting alone, would give a

* *Observatory* (1898), xxi., 404.
† *Observatory* (1898), xxi., 379.

milky sky in the neighbourhood of the Sun in the sky; and the milkiness would not be very different over the centre of the Sun's disc from what it is at, let us say, 5′ or 10′ outside the Sun's limb. On the other hand, scattering by atmospheric tremor, though nearly uniform over the Sun's disc, would fall away very rapidly outside the Sun's limb, and would probably not be very marked at 20″ or 30″ from the limb; it would be evidenced by the "boiling" of the limb and would be greater with large object-glasses than with small.

Some attempts were made in the early part of the year at Cambridge to get from a series of photographs quantitative information as to the brightness of these components of sky-glare. We may express the brightness of the haze by hs, h being the haze coefficient and s being the brightness of the Sun's surface reduced by absorption from its true value S as it would appear outside the Earth's atmosphere. Similarly we may express the brightness of the veil spread by tremor by ts, where t is the tremor coefficient. By spectrum photographs which show a comparison of the Sun's surface with the sky at 5′ from the limb, and by spectrum photographs which show a comparison of the veil and umbra with the sky, we get two equations which would give h and t, if we knew u/s the ratio of the brightness of the umbra to that of the Sun's surface.

If there were no illumined veil but only an absorbent atmosphere between the observer and the Sun, the ratio $u/s = \mathrm{R}$ would be the same as U/S, the true ratio of umbra and Sun's surface seen from outside our atmosphere. But the veil gives rise to an equation of the form (approximate)

$$u + hs + ts = ks$$

where k is the observed ratio of umbra and Sun's surface, seen through the veil. [Evershed measured k under presumably favourable circumstances, and found $k = \frac{1}{20}$.]

We see that to this approximation

$$\mathrm{R} = \frac{u}{s} = k - (h + t)$$

The observations at Cambridge were directed to getting some idea of the order of magnitude of the quantities R, k, h, and t. The results hardly warrant more than hazarded opinions that (1) R the true ratio U/S is more usually nearly $\frac{1}{40}$, and in certain cases it is probably as small as $\frac{1}{100}$; and (2) with regard to the values of h and t, though we found that h ranged from $\frac{1}{15}$ to $\frac{1}{300}$, and that t might rise to as much as $\frac{1}{2}$, without completely obliterating the most marked characteristics of sun-spot spectra, quite usual values for h and t are $\frac{1}{200}$ and $\frac{1}{20}$.

It is evident that the nature of the case is not such as to justify elaborate measurements. The estimates here given are based on comparisons of density of deposit in photographs taken with varied and relatively adjusted exposures, under conditions

chosen for fair comparison. I should add that the observations were carried out mainly in the winter and early spring, generally on days when the seeing was not good enough to encourage the search for special detail in the spectrum but not bad enough to obliterate it altogether.

One point stands out very clearly, namely, that since the characteristic details of sun-spot spectra are discernible, even when the veil of integrated sunlight is as much as perhaps a quarter of the brightness of the Sun's surface, those details must be very marked.

Another point which I would like to emphasise is this:—

The haze component of the veil is probably hardly to be distinguished spectroscopically from true integrated sunlight, except in the relative brightness of widely separated regions in the spectrum. The tremor component, on the other hand, integrates the sunlight only within, let us say, 30″ of the point considered. Thus over the umbra of a spot the tremor component throws a veil of illumination which comes only from the neighbourhood of the spot—that is, from regions which are comparable with the large flocculi exhibited in Hale's spectroheliograms, and which in other parts of these notes I have ventured to call circumpenumbral regions, where the spectroscopic phenomena certainly indicate special disturbed conditions of solar strata. It is obvious that these points have a very marked bearing upon the interpretation which must be put upon bolometric observations of sun-spots.

Differences in the Ways in which Lines are affected.

These preliminary observations have led me strongly to the view that one of the most promising lines of study must be that of the differences between the ways in which various lines are affected in or near sun-spots.

My observations corroborate in very many points those of many observers as to the existence of long lines, short lines, winged lines, etc., such as are epitomised in the drawings of characteristic types given by W. M. Mitchell (*Astrophys. Jour.*, xxii. p. 6, Pl. II.) and by Fowler (*Int. Sol. Research*, vol. i. p. 228, Plate). Sharp dark lines have been seen in the spectrum of the umbra, and also hazy dark lines: some of the dark lines which are fairly strong over the circumpenumbral regions have been seen nearly, if not quite, obliterated over the umbra. But I incline to agree with Professor Hale in requiring further evidence before I am prepared to say that I have seen lines actually reversed into brightness over the umbra. With respect to the *widening* of lines, I am in some doubt; it would appear indisputable that with certain instrumental conditions lines appear to be widened, but it seems to me that the phenomenon disappears when spectroscopes of high powers are used. I revert to this in the next section.

My observations completely corroborate Professor Fowler's to the effect that there are frequent cases where bright intervals between

faint Fraunhofer lines on circumpenumbral regions seem to be continued without diminution of brightness over the umbra. The meaning of the observation is by no means clear.

In my incomplete knowledge of the literature of the subject, I have not yet found whether it is already generally recognised that many lines can be seen to be darkened over considerable regions outside the penumbra. For instance, the lines attributed to iron at wave-lengths

6280·83
6298·01
6301·72

have on several occasions been seen to be darkened over circumpenumbral regions to distances amounting to two or three diameters of a large spot, *i.e.* to distances comparable with two or three minutes of arc.

The extensive flocculi, which Hale has disclosed with his splendid spectroheliographic records, show that the spot proper is but a small part of the whole area of disturbance. We may reasonably expect lines to be affected over regions comparable with the area of such flocculi; and in attempting to get spectroheliographic records in the light of the iron lines, it would be well to distinguish between iron lines which show circumpenumbral darkening and those which show no such darkening.

Of the reality of the phenomenon I have been able to assure myself by observing the spectrum of the circumpenumbral regions without having the spot upon the slit. The darkening of the lines was detected, for instance, in the case of a spot having a penumbral diameter of 40″, and was noted as being quite marked when the penumbra was still 40″–50″ from the slit.

Mr Evershed, whose attention I called to the effect, told me it was one of those phenomena which he had recognised, but had felt unsuitable for systematic recording because the more one tried to fix it, the more it evaded one's power of recording.

It is difficult to know whether to interpret it as a true darkening of the lines, or rather as due to the brightening of the continuous spectrum.

It will be noticed that one of the lines 6301·7 instanced above as exhibiting this circumpenumbral darkening is one of the lines used by Dunér in his spectroscopic determination of solar rotation.

On several occasions I have been able to see in the spectrum of the outer and dark edge of the penumbra darkened lines identical with those seen in the umbra—an observation which ought to be of importance as bearing on our interpretation of the form and level of sun-spots.

It does not seem necessary at present to give the long list of wave-lengths of lines observed as affected lines. Many of these have been recorded by Fowler, Cortie, Mitchell: and my observations support theirs in a vast number of cases. I confine myself

to stating that, after a good deal of preliminary study, I devoted myself to the careful recording of lines affected in the region C–D, as seen on five good observing days, with the equatorial equipment. I have compared these records with the photographic records obtained with the horizontal equipment, and have found that the photographs contain evidence of more "affected" lines than were seen visually. They also contain evidence (which has not been yet systematised or exhausted) of the difference between the ways in which lines are affected over the umbra, the penumbra, and the circumpenumbral regions.

Out of 120 lines which in the visual observations with the equatorial equipment were recorded as "affected lines," I find that

66 are entered as seen in the umbra only.
20 are seen darkened in the umbra and penumbra.
5 are entered as darkened over circumpenumbral regions.
7 are recorded as obliterated.
3 are regarded as exhibiting true widening.
19 appear to belong either to the umbral or to the penumbral group, but the records are indecisive.
10 are recorded as unidentified with Fraunhofer lines.

Photographic records have been secured of the sun-spot spectrum from 5100 to 6700, thanks to the excellent pinocyanol-bathed plates of Wratten and Wainwright. Plate 3 is a photographic reproduction (untouched by hand) of part of one of the original negatives. It serves as a specimen of the kind of material obtained in the red region of the spectrum, where the selective absorption of vanadium is very marked; no less than 10 of the lines which are intensified over the umbra, are due to that element (Rowland's identification).

Remarks on Widening and Darkening of Affected Lines.

Had the observations of sun-spot spectra been restricted to those made with the horizontal equipment and the more powerful spectroscope, I should have unhesitatingly said that the term "widening" was a misnomer. Most of the lines that attract attention as characteristic of the sunspot spectrum are lines that are faintly represented in the (Fraunhofer) spectrum of the neighbouring solar surface. The observations at Cambridge are on this point in agreement with those of Professor Fowler and of Messrs Hale and Adams. But the "affected" lines seem to me to be *darkened* rather than *widened*; so seldom do they appear to be widened when seen through the powerful spectroscope, that I have been tempted to think that the few outstanding cases of apparent widening, would, with higher power, be resolved.

Both visual observations and photographic records afford strong evidence of this. Plate 3 exhibits many instances of lines that stand out as umbra lines and are quite as sharp as any Fraunhofer lines. I would maintain, not that apparent widening does not

SUNSPOT SPECTRUM, PHOTOGRAPHED AT CAMBRIDGE OBSERVATORY 1906 AUGUST 31

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exist, but that widening is not a characteristic peculiar to sun-spot spectra. The widening due to the symmetrical wings of the magnesium lines at *b* is more marked generally over circum-penumbral regions than over the umbra, according to my observations this summer; whilst the reverse is more generally true of the sodium lines at D. The three lines recorded in the last section as exhibiting true widening are all calcium lines.

A good typical instance of what I refer to is afforded by the line at 6306. This line has been described as "generally much widened." It is often entered as due to terrestrial oxygen, Rowland's identification of the line 6306·024 being accepted. Professor Fowler has ascribed it to scandium. My observations of the line with the equatorial equipment supported the idea of true widening (in spite of its unsymmetrical nature) to such an extent that I find in the diary notes the following entry: "1906 July 18. Umbra line at 6306 studied carefully. This is a strong line and sometimes looks like a much widened line, sometimes looks more as if it [Rowland's A(O) line] had a dark line near it. To-day it certainly looks like true widening, the only case of widening that I am quite sure of yet."

It hardly needed more than a glance with the more powerful spectroscope of the horizontal equipment to decide the case against widening. The fact is, a new line appears in the spectrum of the umbra on the violet side of the oxygen line. Even the photographic records show the line as double. Measurements give the wave-length of the umbra line as 6305·86, whilst the A(O) line has wave-length 6306·02. Rowland gives a Fraunhofer line of intensity 0000 at wave-length 6305·878. It would be interesting if this turns out to be the scandium line which Professor Fowler finds in Thalén's tables.

These details have been given because they bear on the general question of "darkening" *versus* "widening" of sun-spot lines; they bear also on the curious coincidence of telluric lines with affected lines in the sun-spot spectrum, to which Father Cortie and other observers have referred.

Bands in the Spectra of Sun-spots.

Suspected Fluting in the Green Region, 5000–5168.—There are many isolated lines confined to the umbra (*e.g.* vanadium lines) which cannot be yet put into the category of elements of a band or fluting; for criterion as to whether a given line is isolated or an element of a band, we require analysis such as Alexander Herschel was the first to suggest and to apply, and such as Deslandres has applied with such success to the banded spectrum of nitrogen, etc. Still, as a first attempt, the observer is led by a sort of instinct to pick out lines as belonging to a band; and in my earlier observations in 1905 December and 1906 January, I devoted some time to the study of umbra lines in the region 5100 to 5168, estimating

wave-lengths with a view to attempting to determine the probable chemical origin of the fluting.

It was at once evident that my observations confirmed Fowler's view that the lines which seemed to be specially characteristic of the sun-spot spectrum were in reality also feebly represented in the solar spectrum.

The lines do not agree in position with Rowland's carbon lines, nor can they be made to agree either by the assumption of some displacement common to all, or by the assumption that the observed dark lines are intervals between bright lines.

I expected, by plotting the estimated positions of the lines carefully on an enlarged reproduction of Rowland's map, drawn (on a scale of 1 cm. to 1 tenth metre) on a paper millimetre scale, to get values of wave-length trustworthy to the tenth of a tenth metre—accuracy sufficient for identification of the fluting. The recorded positions were read off with care to the hundredth of a tenth metre, and I intended to discard the second decimal. But a comparison of my results with the mean values which were published shortly afterwards by Hale and Adams, as determined from their beautiful photographs of sun-spot spectra,* showed such unexpected precision that it seems well to record the experience for the benefit of observers who might use the method in the absence of refined micrometers.

Photographic Measures, Hale and Adams.	Visual Estimates, N.	Photographic Measures, Hale and Adams.	Visual Estimates, N.
5135·83		5151·37	
	36·50 } pair	51·63	
36.65	36·65 }		52·28
38·58	38·59	52·82	
38·96	38·97	56·53	56·53 } pair
40·44			56·74 }
41·40	41·48	57·20	
	43·60 } pair	57·79	
43·83	43·81 }	58·81	
44·21		59·96	59·92 } pair
45·93			60·11 }
	47·92	60·39	
48·93	48·89		62·90 } pair
49·67		63·10	63·11 }
49·99		63·79	63·59
50·36	50·30 } pair		
	50·55 }		

My attempts to find a known fluting that will correspond to the lines recorded in the table have been hitherto unsuccessful.

* *Astrophys. Jour.* xxiii. 11–44, 1906.

My estimates of the wave-lengths agree more closely with Hale and Adams' measures than with Mitchell's observations.

Suspected Flutings in the Red Region, 6370–6390.—The groups of lines recorded by Young, Mitchell, Cortie, and Fowler, as seen in the red region of the spectrum about wave-length 6381 and 6388, have been studied carefully on several occasions; and the set of observations made with the equatorial equipment on July 30, 1906, have given the following data for 21 lines between wave-lengths 6377–6393. The measures given in the last column were made on a photograph (R.S., 208) taken 1906 August 31.

Visual Estimates.	Intensity.	Photographic Measures.
6377·81	Dark line 3	·76
78·15	Narrow bright streak	
78·48	Dark line 3	·47
78 94	,, 4	·99
79·64	,, 3	·47, ·94
80·31	,, 4	0·94
81·83	,, 3	1·60
82 33	,, 4	·37
82·65	,, 2	·75
82·84	Limits of bright streak	·84
83·75		·55
84·04	Dark line 2	3·98
84·43	,, 2	·36
84·84	,, 3	·86
85·10	Limits of bright streak	4·96
85·45		5·26
85·72	Dark line 4	·82
6386·06	Dark line 4	·11
86·76	,, 3	·57, ·79
87·57	,, 3	·27, ·77
88·46	,, 3	·36, ·60
89·02	,, 3	·00
89·56	,, 4	·18, ·48
89·73	Limits of bright streak	·68
90·58		·39
90·82	Dark line 3	·49, ·81
91·10	Limits of bright streak	0·99
91·44		
92·57	Dark line 3	·76
93·18	,, 2	·20
93·84	Unaffected Fraunhofer line, intensity 6 on same scale	

These groups often have the appearance of bands degraded towards the violet side and with heads at

	6382·33	and at	6389·56
Cortie gives the values	6380·96	and	6388·63
Fowler gives	6381·6	,,	6390·0

The banded appearance is naturally least marked when the lines are seen most distinctly. From the estimates of intensity given in the table above, it would be difficult to pick out the "heads" of the bands.

On two occasions towards the end of my observations in last August (1906), there were signs of another green fluting degraded

towards the violet, and with its head not far from wave-length 5210. Attempts to determine the position of the head on two different days led to the following results:—

1906, Aug. 11	5210·2
,, ,, 21	5211·0

This dark hypothetical fluting is recorded "as quite marked at times and not an illusion due to the proximity of Fraunhofer lines, for the E group is not so affected. The part of the spectrum between 5210 and 5200 is the darkest part of spot spectrum in the region between 5100 and 5300." This fluting is at a region where there are many umbra lines, and had the appearance of a darkening of the unresolved background of the spot spectrum.

Laboratory Experiments on Resolution of the Background of Arc Spectra.

Probably all spectroscopists have realised that, in studying the arc spectrum of, let us say, iron, they concentrate attention upon the bright lines of iron and consent to disregard the "continuous background" on which the lines are generally seen. When, however, powerful spectroscopes are used and attention is paid to the background, it is seen to be resolved into fine lines under conditions which are generally not quite under the control of the observer. In many cases the lines show signs of arrangement in bands and flutings; in other cases the signs of structure of that kind are not detectable.

The resemblance of this varied structure in the background of bright arc spectra to the structure that is visible under what we are tempted to call the finest conditions of seeing in the spot spectrum, must have struck many observers. At any rate it caused me no surprise when, in the spring of this year, I gathered from Mr Fowler that he had been looking for detail in arc spectra similar to that which had led him to identify some flutings in star spectra with the reversal of the flutings discovered by him in the titanium spectrum and attributed by him to oxide of titanium.

I have, both before and after my conversation with Mr Fowler, secured a few photographs of the background of some arc spectra with a very powerful grating spectroscope, and have compared some of the details exhibited on the photographs with the fine structure of the sun-spot spectrum. So far I have not had much success in finding similarity. But in one case I find resemblances which call for further investigation of the sort; for bright flutings degraded towards the red have been found in the spectrum of the arc between iron terminals, which had been tipped with lime,—flutings which coincide in position and direction of degradation with unsymmetrically winged lines darkened in the sun-spot spectrum close to the wave-lengths (*a*) 5504 and (*b*) 5528.

I should add, however, that I have found no flutings recorded by other observers of sun-spot spectra at these wave-lengths.

Mitchell's observations agree with those of Hale and Adams with respect to darkened lines near one position (*a*), but not in the second case (*b*).

	Mitchell.		Hale.	
(*a*)	5504·12 Ti	Darkened	04·12	
	06·10 Ma	Generally much darkened	06·10	
	07·00 Fe	Winged	07·00	Winged
(*b*)			20·73	
	5522·66			
	25·77	Hazy		
			27·80	
			29·38	
	30·99	Darkened		

Search for Evidence of Local Convection Currents on the Sun's Surface.

On many occasions when we were using the equatorial equipment, special attention was given to a search for violent motions in the line of sight in the neighbourhood of sun-spots. In 1895 I spent five weeks at the Riffelberg, making spectroscopic observations of the Sun's surface with a grating spectroscope. Several times I found evidences of violent local downrushes of hydrogen, which were detected by what one may call isolated islands of the C line, showing large displacement towards the ultra-red. These downrushes of hydrogen were indicated by a dot in the spectrum, and the absence of other general darkening of the spectrum proved that they were not connected with any *formed* sun-spot. They persisted on two or three occasions for many hours, and could easily be picked up by allowing the solar image to trail across the slit of the spectroscope; the small black island of C line would suddenly flash out and in another moment be gone, unless the trailing of the image were stopped. Some correspondence that I had with Dr Vogel and Father Sidgreaves at the time, led me to the view that the phenomenon was unusual. I had, without great success, tried to connect the positions of the downrushes of hydrogen with positions in which spots appeared later.

I made further attempts during the past summer to detect similar phenomena, but though I regularly examined the Sun's surface in this way with the equatorial equipment, though the operation of trailing the whole surface of the Sun past the slit took 30 minutes to complete on each day that it was examined, I never could detect a single instance of the occurrence of a localised downrush. It may be pointed out that it is just eleven years—

the period of the sun-spot cycle—since the previous observations at the Riffelberg disclosed the apparent frequency of the phenomenon.

The observations, however, served to show also how seldom it occurs that large disturbances of the kind that would be detected by distortion of the lines in the spectrum are to be seen on the Sun's surface. Belopolsky has called attention to this point in his note published in vol. i. of the *Transactions of the Union for Solar Research.* If this observation is further corroborated, it would tend to show that the violent convection currents, which we believe are needed to keep up the supply of radiation from the Sun's surface, and which are so well brought to mind by Professor Schuster in his Glasgow lecture on solar evolution (*Astrophys. Jour.*, xvii. 173), must be confined to strata below the reversing layer. This is a result which it seems difficult to reconcile with other considerations.

I have great pleasure in acknowledging my obligations to Mr W. H. Manning for his efficient help in these observations under circumstances that called for much patience.

To Mr J. B. Hubrecht also, whom we are glad to have at the Observatory, working as a research student in astrophysics, I would here render thanks for his help on many occasions when he has been at hand making preliminary observations for a determination of the rotation of the Sun, to be derived from photographic records taken in the spectroscopic method.